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DISCUSSION ERRATA

Discussion of papers published in the April through June, 1961, JOURNALS appears in this issue, along with title page, errata, contents, and indexes for Proceedings V. 57, thereby completing this volume. Discussion of papers published in the July through September, 1960, JOURNALS appeared in March, 1961; discussion of papers published October through December, 1960, appeared in June 1961; discussion of papers published January through March, 1961, appeared in the Part 2 September, 1961, issue. Discussion pages in this issue are numbered continuously from the final page of the Part 2 September issue.

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ERRATA and ADDENDA

The following additions and corrections should be made in the indicated papers and reports of *Proceedings* V. 57, July 1960 through June 1961.

* * * *

Title 57-4 "Shear Strength of Restrained Concrete Beams Without Web Reinforcement"

p. 77 — heading for the fifth column of Table 1 should read: $\frac{P_s}{P_1}$

* * * *

Title 58-11 "Tests of Rigid Frame Bridge Model to Ultimate Load"

p. 232 — in Table 3 the reading under Column "WID-6" for Test II-B, 9400-lb load, should be changed from —1 to —8.

* * * *

Title 57-12 "High Strength Deformed Steel Bars for Concrete Reinforcement"

p. 248 — the column heading "Deformations at load approaching failure load," at the right side of Table 2, should cover only the last three columns at the right side of the table, and the column listed as "Type Failure" should be a separate entry and should not be part of the above heading.

254 — in Eq. (3), change that part of the denominator from Q to Q_s and in the line immediately under this change that portion from ". . . . and Q_s refer to . . . ." to read ". . . . and Q_s refer to . . . ."

p. 256 — the line immediately above Eq. (4), which reads ". . . . measured stress versus . . ." should read ". . . . measured stirrup stress versus . . . .".

p. 257 — for clarification of Fig. 4, the ordinate axis, f_s, is the measured stirrup stress."

p. 261 — change the first two lines immediately under Table 4 to read— "The ratios of actual ultimate moment, M_u, to calculated moment, M_f, given in Table 4 indicate . . . ."

p. 267 — change the last line at the bottom of the page which reads ". . . . II D—1 M." to read ". . . . III D-1M."

p. 269 — change the seventh line from the bottom of the page (excluding footnotes) to read ". . . . at failure, V_u, to . . . ."

p. 272 — change the fourth line from the top of the page which reads ". . . . value of $\overline{M}/M_f$ is . . . ." to read ". . . . value of M_u/M_f is . . . ."—and in the first and fourth lines from the bottom of the page, change the V_u in both cases to read V_u.

p. 273 — in the second line change "V_u" to "$\overline{V}_u$."

p. 279 — in the sixth line from the bottom change "with ⅛-in." to "within ⅛-in."

* * * *

Title 57-16 "General Formulas for Membrane Stresses in Hyperbolic Paraboloidical Shells"

p. 359 — in the second line of the second equation for v_x, change "5/5 k sin ωx_1" to "— 5/4 k sin $^2\omega_1$."

p. 360 — in the second line of Eq. (19c), change "1/2 $Z_1 y$" to "1/2 $Z_1 x$"

p. 361 — in the second line below Eq. (22c) change "$\theta = \omega/2$" to "$\phi \approx \omega/2$"

p. 364 — correct the following equations to read:

$$\bar{\sigma}_x = \sigma_\beta \frac{\cos(\beta - \alpha)}{\sin \beta} + (\tau_\beta - \tau) \frac{\sin(\beta - \alpha)}{\sin \beta} \quad \text{(25a)}$$

$$\bar{\sigma}_y = -\sigma_\beta \frac{\cos \beta}{\sin(\beta - \alpha)} - (\tau_\beta + \tau) \frac{\sin \beta}{\sin(\beta - \alpha)} \quad \text{(25b)}$$

p. 366 — in Table 1, Column Headings 1, 2, and 3, change Y, X, and Z, to y, x, and z respectively.

* * * *

Title 57-21 "Determinations of Membrane Stresses in Elliptic Paraboloids Using Polynomials"

p. 436 — in the formula for t in Eq. (3e), change "$\partial^2 F/\partial x \partial y$" to "$-\partial^2 F/\partial x \partial y$."

p. 437 — in the last line of Eq. (3g), change "Eq. (3f)(c)" to Eq. (3f)(b)."

p. 437 — in the first line of the last paragraph change "Eq. (3c)" to "Eq. (3e)."

* * * *

Title 57-28 "Reactivity of Ultrafine Powders Produced from Siliceous Rocks"

p. 569 — in Reference 10, change 1959 to 1955.

* * * *

Title 57-35 "Resistance to Shear of Reinforced Concrete Beams. Part 5—Anchorage and Bond"

p. 715 — in the notation change o to Σo.

p. 721 — in the last sentence of the second paragraph change 190 to 1.90.

p. 723 — in the ninth line from the bottom of the page change Σ_o to Σo.

* * * *

Title No. 57-37 "Investigation of Compressive Strength of Molded Cylinders and Drilled Cores of Concrete"

p. 773 — change "$\sigma \cdot ^y = \sigma_b$" to "$\sigma \cdot y = \sigma_b$."

* * * *

Title No. 57-38 "Freezing and Thawing Tests of Lightweight Aggregate Concrete"

p. 784 — in Table 4, second column from the right, change "weight" in the heading to "volume."

p. 793 — in the abscissa to Fig. 5 change "weight" to "volume."

* * * *

Title No. 57-46 "Rheological Behavior of Hardened Cement Paste under Low Stresses"

p. 949 — in the third paragraph change Fig. 7 to Fig. 6.

* * * *

Concrete Briefs section "Ultimate Strength Design of Beams with the Aid of Tables"

p. 980 — under the first radical in Eq. (4) the material in brackets should read "$(1 - 0.59\, p\, f_s/f_c')$" instead of "$(1 - 0.59\, f_s/f_c')$."

• • • •

Title 57-52 "Structural Models Evaluate Behavior of Concrete Dams"

pp. 1120 and 1121 — interchange the captions to Fig. 13 and 14.

• • • •

Disc. 57-13 "Effects of Aggregate Size on Properties of Concrete"

p. 1234 — In the seventh line of the third paragraph change "water-cement ratio" to "cement factor."

p. 1234 — In the 11th line of the third paragraph delete "water-cement ratio" and insert "cement factor (450 lb per cu yd)."

p. 1234 — Fig. N referred to in the eleventh line of the third paragraph was inadvertently omitted. Fig. N appears below.

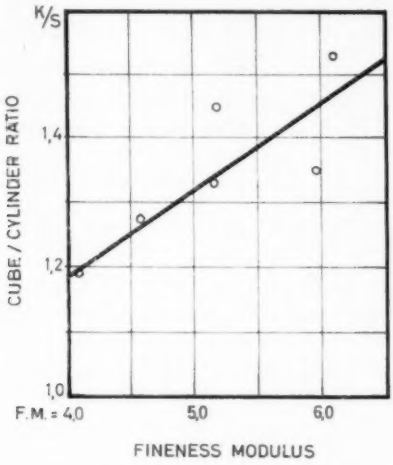

Fig. N—Gyenco test results

• • • •

Disc. 57-35 "Resistance to Shear of Reinforced Concrete Beams"

p. 1691 — at the bottom of the page change the second last equation "$M = 20000 \times 77 = 1{,}540{,}000$ in.-lb" to "$M = 20{,}000 \times 70 = 1{,}400{,}000$ in.-lb."

p. 1692 — in the fourth and fifth line change "$M = 25000 \times 92.7 = 2{,}310{,}000$ in.-lb" to "$M = 25{,}000 \times 91.0 = 2{,}280{,}000$ in.-lb."

p. 1692 — in the second line of the second paragraph, the second line of the third paragraph, and the fourth line of the last paragraph, change "V_u" to "v_u."

p. 1693 — in the third line change "f_c/v_i" to "f_c/v."

p. 1693 — in the fifth line change "f_c'" to "f_c."

p. 1694 — in the fourth and fifth lines from the bottom delete "the two limit cases and are derived from."

p. 1695 — in the third line of the second paragraph insert "and V_c" after ". . . T and C'."

p. 1696 — in the ninth line change "bending up" to "cutting off."

p. 1696 — in lines 13 and 14 delete "or bent up."

p. 1697 — in the fourth line of the third paragraph change "Beams 38 and 47" to "Beam 47."

p. 1697 — in the third line from the bottom change "chose" to "chosen."

p. 1698 — in the fourth line change "appearance of a crack" to "total destruction."

p. 1699 — in the seventh and eighth lines from the bottom change "ultimate theory" to "ultimate theory of simple bending."

p. 1701 — in the third line of the first paragraph under the heading NUMERICAL EXAMPLE insert "each" after "22 kips."

p. 1702 — in the fourth line change "Eq. (14c)" to "Eq. (14d))."

p. 1702 — in the seventh line from the top change "920,000/5" to "92,000/5."

p. 1702 — in the eleventh line change "K_2" to "k_2."

p. 1703 — in the first and second lines change "$Q_m > bd\tau_u$" to "$V > bd\tau_{cu}$."

p. 1704 — in the eighth line change "(14c)" to "(14d)."

p. 1704 — in the middle of the page change "$K = \frac{vV}{bdf_c}$" to "$k = \frac{vV}{bdf_c}$."

p. 1704 — just above Eq. (18) in the middle of the page change "Eq. (14c)" to "Eq. (14d)."

p. 1705 — in the fourth line of the fourth paragraph change ". . . distribute the compressive force C' use the shear v or friction factor of concrete." to ". . . distribute the compressive force of C' by v the shear or friction factor of concrete."

p. 1725 — in the third line of the second paragraph under "AUTHORS' CLOSURE" change reference 1 to 65.

p. 1725 — in the first line of the third paragraph from the bottom change reference 2 to 66.

p. 1726 — in the eleventh line of the third paragraph change "the authors did feel that any generally applicable statement could be made" to "the authors did *not* feel that any generally applicable statement could be made."

p. 1727 — in the last line of the second paragraph change reference 3 to 67.

p. 1727 — in the sixth line of the last paragraph change "it" to "t."

p. 1728 — References 61, 62, and 63 should be changed to 65, 66, and 67, respectively.

* * * *

Disc. 57-46 "Rheological Behavior of Hardened Cement Paste under Low Stresses"

p. 1799 — 7th line from the bottom change "K" to "$\overline{K}$."

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Discussion of a paper by R. O. Hedstrom:

Load Tests of Patterned Concrete Masonry Walls*

By R. E. COPELAND, C. C. FISHBURN, and AUTHOR

By R. E. COPELAND†

Mr. Hedstrom's paper is a valuable contribution of new and long needed information on the subject. It obviously represents the results of a carefully planned and conducted reseach of major magnitude. The following comments are intended to elicit additional data, if available, and discussion on a few aspects of the investigation.

Table 2 indicates the safety factors of the walls in compression, these factors being derived by dividing the ultimate average unit compressive strength of the wall by an allowable maximum stress of 85 psi. Since wall strengths were determined under eccentric loading it would be more correct to relate the safety factors to the allowable average rather than maximum stress which in this case would be about 57 and 50 psi for the walls built of Types M and S mortars, respectively. This results in a 50 percent increase in the safety factors from the values shown but this is on the basis of the relatively high strength block of which the walls were constructed. If the walls had been built of block just meeting the minimum compressive strength requirement of 700 psi, the safety factors calculated as suggested would be about 75 to 85 percent of the values shown in the table.

It is noted that the stronger, Type M mortar exhibited considerably higher tensile bond than the Type S mortar, both in the block couplet tests and the flexural tests of walls in the vertical span. The flow and air content of the mortars would be of interest since these properties are presumed to influence mortar bond.

Comparing the compressive strengths of vertical and horizontal stack bond walls and of individual block tested in the vertical and horizontal or normal position, it appears that the strength values varied substantially in direct proportion to the net compressive area perpendicular to the direction of stress and was not appreciably influenced by other aspects of the orientation of the block. This relationship provides a rational and convenient basis for determining safe design stresses for vertical stack bond construction; considering typical block such stresses would be about 35 percent less than those allowed for running bond or

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1265.

†Member American Concrete Institute, Consulting Engineer, National Concrete Masonry Association, Wheaton, Ill.

horizontal stack bond walls. The writer believes it will be more practical to vary the allowable design stresses with the net compressive area than to depend on the alternative solution, that of requiring a special strength grade of block.

It is noted that most of the walls tested for transverse strength while simultaneously stressed in axial compression continued to carry an increasing transverse load after rupturing in flexure. After rupture, the resisting moment due to the superimposed plus dead load compressive stress theoretically did not exceed about 31,600 in.-lb yet some of the walls sustained transverse loads equivalent to a moment of 35,000 in.-lb if simple supports are assumed. The base support is not described but presumably it was the usual fixed flat bearing which does not allow the wall to rotate freely at the base. Instead, as the wall deflects, the resultant of the vertical reaction moves toward the outer edge thus causing a favorable shift in the load line and arm of the compressive stress force couple. This may account for the difference between the actual and theoretical ultimate transverse load values.

It also is difficult to reconcile the theoretical and actual horizontal span flexural strength of the walls containing joint reinforcement spaced 16 in. on center. According to the writer's calculations the reinforcement at the tension face of these walls was stressed to from 142,000 to 210,000 psi at the indicated ultimate loads as compared to a tensile strength of 100,000 to 110,000 psi for the wire usually used in such reinforcement. Similar calculations for the walls built with joint reinforcement spaced 8 in. on center indicated that the steel stresses at failure were in the more reasonable range of 120,000 to 130,000 psi. Perhaps the author can contribute additional information bearing on this aspect of the investigation.

Regarding the diagonal basket weave and diagonal running bond patterns which in the compression tests failed in mortar bond, additional tests involving weaker mortars may be desirable. In other respects the investigation should adequately meet present needs for information on the subject.

By C. C. FISHBURN*

The author contributes an excellent and interesting discussion on the effects of differences in bonding patterns on the compressive and transverse strength of concrete masonry walls.

The agreement between the bond strength of block assemblies and the flexural strength of walls built in running bond indicates the importance of bond strength on the flexural strength of unreinforced concrete masonry. A similar relationship between the strength of bond

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test specimens and the flexural strength of walls was noted in an investigation on the properties of masonry mortars recently completed at the National Bureau of Standards.

It would be interesting to this reader to have some information on the initial flow and water retention values of the mortars as used by the mason.

AUTHOR'S CLOSURE

The author would like to thank Mr. C. C. Fishburn and Mr. R. E. Copeland for their discussions.

Mr. Fishburn reaffirms the importance of bond strength on the vertical span flexural strength of unreinforced concrete masonry in the work he has done at the National Bureau of Standards. This same importance of bond strength does not appear in the tests made across the horizontal span except in the stacked bond patterns. The information requested on the initial flow and water retention values of the mortars as used by the mason is given in Table A.

Mr. Copeland inquired whether the flow and air contents had an effect on the bond strength of the two mortars. Table A shows only small differences in initial flow and in air content of the two mortars. Important differences in bond strength cannot be attributed to the small differences in these variables. The mason adjusted the mortars to the desired workability so that the difference in flow does not directly represent difference in workability.

Mr. Copeland suggests that the factors of safety reported for these tests should be calculated using the 85 psi allowable stress as a maximum stress, rather than an average stress, since the compression load was applied eccentrically. The American Standard Building Code Requirements for Masonry does not clearly state whether the allowable stresses are based on concentric or eccentric load with respect to hollow block walls. However, the Code specifies that wall panels should be tested in accordance with ASTM E-72 which requires an eccentric load. The author interpreted this to mean that the allowable stresses were also based on an eccentric load. If the stresses allowed by the Code can

TABLE A—INITIAL FLOW AND WATER RETENTION VALUES OF MORTARS

Mortar	Average initial flow, percent	Range of initial flows,	Water retention, percent	Air content, percent
1:1¼:4¾	133	118 to 145 (129 tests)	86 to 92	3½
1:2:7	123	112 to 134 (73 tests)	94	3

be interpreted to refer to concentric loading, safety factors could be increased about 25 percent as established by University of Illinois tests.[3] On this basis the allowable loads are conservative when a Grade A block is used. The Appendix to the Codes does contain a sentence (Section 4.2) which might be interpreted to substantiate Mr. Copeland's thought, ". . . working stresses in compression have been selected so that the minimum safety factor with concentric loading is about 3½."

The compressive strength of a masonry wall is of only minor importance in most residential and single story commercial construction. A wall built with units which are at the lower end of the strength requirement for Grade B block (700 psi) will be safe as far as its load carrying capacity is concerned. Of greater importance than their load carrying capacity, the weaker units have higher absorption, lower tensile strength, and are more easily damaged, all of which contribute to easier water penetration and generally poorer construction.

From these tests of the units, it appears to be true that the compressive strength of the units based on *net* area was approximately the same for a block tested in either a horizontal or vertical direction. Full mortar bedding was used for individual block tests. However, the strength of the wall was not proportional to the net areas of the block in the two directions. Face shell bedding was used in the walls so that the strength of the wall might be expected to be the same whether the block were horizontal or vertical. The fact that the walls with horizontal block were about 20 percent stronger than those with vertical block may be explained by the fact that the webs were partially bedded and in addition to the increased bedded area the webs also aided in stress distribution.

Mr. Copeland's assumption regarding the transverse load test with superimposed compressive load is correct and a proper accounting for the loads obtained.

Mr. Copeland had difficulty reconciling the theoretical and actual horizontal span flexural strength of the walls containing joint reinforcement. The explanation for the apparently inconsistent results is that the center of moments does not remain fixed in this type of test. The author did not measure this translation when he made these tests. However, in current work on sandwich wall panels (two wythes of structural concrete with a panel of insulating material in the middle), it has been shown that the neutral axis, although at the center of a symmetrical panel to start, moves toward the compression face as the load increases and as tension cracks appear in the tension side of the test panel. This movement of the neutral axis from the original point is large in lightly reinforced panels but decreases as the panel becomes stiffer or more heavily reinforced. In the sandwich wall panels the measurements and observations of crack propagation indicate move-

ment of the neutral axis to within ¼ to ⅜ in. of the compression face of the panel.

In the masonry walls with joint reinforcement every 16 in., the movement of the neutral axis only as far as the center of the compression steel reduces the stress in the tension steel to less than the tested strength of the steel we used, namely, 91,000 psi. The walls with the steel spaced every 8 in. were stiffer and therefore the neutral axis did not move as far before the tensile strength of the steel was reached and the walls failed. In the walls with steel spaced at 16 in. center to center where the transverse strength was over 130 to 135 psi the added strength was due to the concrete in the tension face. In one running bond patterned wall the strength went up to 149.4 lb per sq ft before the concrete failed in the tension face. After the concrete cracked, the load immediately fell off and since the steel had not been broken, load was again applied to the wall and when this second loading reached 130 lb per sq ft the steel failed as it did in the horizontal stacked bond walls where the mortar bond had failed at a much lower load.

Discussion of a paper by Herbert J. Gilkey:

Water Cement Ratio Versus Strength – Another Look*

By ROBERT A. BURMEISTER, HERBERT K. COOK, J. HOLLEB, L. LIBERTHSON, INGE LYSE, M. SPINDEL, STANTON WALKER and DELMAR L. BLOEM, G. B. WELCH, WALTER H. WHEELER, HUBERT WOODS, and AUTHOR

By ROBERT A. BURMEISTER†

Professor Gilkey is to be commended for his excellent and thought provoking paper on a nettlesome problem — namely, what is the significance of the water-cement (W/C) ratio? The writer has been one of those classified by Professor Gilkey as a disbeliever of the so-called W/C ratio law for a long time. This attitude came about after laboratory experience in both the City of Milwaukee Testing Laboratory and the Wisconsin Highway Commission Laboratory. During the early thirties, Guy H. Larson, then head of the Highway Laboratory, and the writer had occasion to proportion mixes for paving concrete for hundreds of aggregate sources which varied from limestone to igneous rock, rounded gravel particles to crushed stone. It gradually became apparent that strength was not a function of the W/C ratio, the most generous interpretation of the so-called W/C law was that there was a vague trend. Later it was found that durability was also not a function of the W/C ratio, again there being only a vague trend. It should be remembered that in those days it was bluntly stated by some concrete technologists that both strength and durability were primarily dependent on the W/C ratio, although formal statements of the law had qualifying clauses such as "within ranges of workability" or "other things being equal." It also should be remembered that various bulletins and manuals showed tables or curves where probable strengths were shown in terms of the W/C ratio and where durability was also inferentially shown in terms of the W/C ratio in that a lower maximum ratio was prescribed for severe exposure conditions than for mild exposure conditions.

Some technologists also found merit in the W/C ratio concept in that it afforded a way of measuring or controlling water content of the concrete. While it is obvious that the water content (and of course the other ingredients) should be closely controlled, to do so by expressing

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1287.

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a ratio rather than by direct measurement seems highly illogical to the writer.

The mix proportioning alluded to herein involved four batches (of different W/C cement ratios) per aggregate source, each batch weighing 300 to 400 lb. The cement used in these mixes was originally a mixture of three current brands then in wide use in the State Paving Program and which were evaluated by continual tests amounting to several thousand samples per year. To assure a constant quality source of supply of cement for the concrete mix proportioning program, a large quantity of special laboratory blend was made of these three brands and stored in a silo. Later it was found that the blend gave concrete strengths that were sometimes not attainable in the field, hence it was then decided to use the lowest strength producing brand as a basis for mix proportioning. Recently Walker and Bloem* have convincingly demonstrated that the matter of variation of strength producing quality in cement is of paramount importance. Tests of cements in the Milwaukee area in the past several seasons have shown that variations in strength producing qualities of cements are considerable but are not as extreme as shown for Cement No. 5 in the Walker and Bloem paper. For example, one brand had a season low of 2320 psi and a season high of 3250 psi (7 day C 109 cube strengths, mill tests).

From these experiences it appears to the writer that the fundamental fallacy in expressing properties of concrete in terms of a ratio is that that ratio varies in significance from job to job, from brand to brand of cement, and from day to day even using the same brand of cement; hence no valid law can be established.

But there is another fallacy in the W/C ratio law and that is the unwarranted preoccupation with just two of the ingredients of concrete, viz., water and cement. What about the effect of the other two ingredients, viz., fine and coarse aggregates? In the early concept of the W/C ratio law, the aggregates were conceived of as inert fillers and so long as they met the ASTM aggregate specifications one source was regarded just as good as another. It was soon found that when job curves were plotted that there were significant differences in strengths of concrete made with various aggregates; hence the W/C ratio strength curves shown in various bulletins were expressed as bands. It has been the writer's experience that these bands are so wide they lose their meaning. In addition, the inception of air entrainment phenomena gave a new meaning to concrete durability.

As to Professor Gilkey's cogent reference to isotropicity, the writer remembers a long ago conversation with M. O. Withey of the College of Engineering, University of Wisconsin. The question was why certain

*Walker, Stanton, and Bloem, D. L., "Variations in Portland Cement," *Proceedings*, ASTM, 1958.

test results of concrete were not coherent and Withey replied to the effect that coherence is not always attainable with a subject (concrete) that is basically nonhomogeneous, having ingredients of varying sizes, shapes, strengths, surface characteristics, moduli of elasticity and coefficients of expansion.

In the past two decades the writer has had an active role in quality control of concrete for a multitude of paving, building, bridge, and sewer projects altogether involving several million cubic yards of concrete and has not used the *W/C* ratio as a criterion for either strength or durability.

By HERBERT K. COOK*

Professor Gilkey has always been held in the highest esteem by the members of the concrete fraternity. It would not be correct, even in this instance, to label him as "the great dissenter," since while he indicates with factual evidence that blind acceptance of the *W/C* pronouncement as originally expressed is a fallacy, he agrees that the *W/C* ratio is the dominant consideration in the design of concrete mixtures within the general range of essentially similar materials. Nevertheless, it has been he, perhaps more than any other individual associated with the concrete industry, who has acted as the invaluable counterbalance or gyroscope and kept many of us, in our zeal, from taking the wrong course or from becoming hopelessly lost. In view of his many other contributions to the advancement of concrete it would be presumptuous to say that this is his greatest contribution. Certainly, however, his carefully composed and well documented questioning of the currently accepted relationship between *W/C* ratio and strength is a valuable contribution to the field of concrete. It would be well for all concerned with this important construction material to take "another look" at the subject of his paper as well as at some of the other commonly accepted criteria of concrete mix proportioning and concrete quality.

The writer is in agreement with Professor Gilkey's suggested revision to the wording of the *W/C* versus strength pronouncement, as stated in Item 5 of the Summary of his paper, insofar as it goes. The phrase "insofar as it goes" in this latter sentence by no means implies that it should go farther, but is intended to imply that it is not possible to include all of the many complex variables contributing to the properties of concrete in a single simple statement.

The writer also applauds the restraint, which he is sure is deliberate, in referring to the *W/C* pronouncement as a pronouncement rather than to the too common reference to the *W/C law*. In fact, it is probably because of the practice of referring to this relationship as a law that it now appears desirable to take another look at its implications.

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Exception is taken to the use of the word "stiffness" as applied to aggregate in Item 5(c) of Professor Gilkey's suggested revision. It is believed that hardness or strength and probably particle shape of the aggregate influence the strength of the concrete. Possibly these properties are implied in the qualification that the aggregates must be acceptable but the intent of the property indicated by stiffness is not clear and should be clarified.

It is noted that both Abram's original pronouncement and Professor Gilkey's suggested revision start with the words, "For a given cement . . ." This, of course, is proper since it is well known that all cements do not produce identical strengths, all other materials and mix proportions being constant. However, it does raise the question of the desirability of also taking another look at the common practice of specifying minimum cement factors and usually maximum W/C ratios. Cement and water content for a given strength are interrelated. This is the basic factor in the original W/C versus strength pronouncement. Professor Gilkey points out the fallacies inherent in the pronouncement as originally stated and indicates the more important of the other conditions that also affect strength. It is apparent that in the design of a concrete for a desired strength these factors will also affect cement content. Furthermore, it is believed that while many are aware of the effect of the W/C ratio on strength as indicated by Abram's original pronouncement, some at least have forgotten that the pronouncement starts with the words, "For a given cement." There is a general tendency to feel secure in requiring a minimum cement content and a maximum W/C ratio and feeling assured that everything possible has been done to insure that concrete of the desired strength and of high quality will be the result.

There seems to be a too prevalent tendency to accept general statements as law. The W/C ratio versus strength relationship, the effect of sand grading (fineness modulus) on concrete proportions, the proportioning of concrete mixes based on minimum cement content, the effect of maximum size of coarse aggregate on concrete strength, the effect of air content on the strength of lean and rich concrete mixes are a few examples. All of these relationships are helpful guides to the concrete engineer but they are only guides, and while they serve as excellent starting points they are no substitute for a proper engineering evaluation of the optimum proportions and properties of concrete made with specific job materials. Such an engineering evaluation is a routine operation for large jobs. Smaller jobs to a great extent are furnished by a ready-mixed concrete producer who is in the best position of anyone to know his materials and optimum mix proportions for a wide variety of classes of concrete. Certainly, if he operates on an adequate quality control basis, which he can hardly afford not to do, he

should develop a whole series of W/C ratio curves to best fit the materials and classes of concrete with which he operates. He would do well to profit from the limitations of the W/C pronouncement so ably discussed by Professor Gilkey.

Concrete is the most versatile of all construction materials. Those of us who have made it our career are concerned about maintaining its dominant position in the construction industry. To do this we must, on a sound engineering basis, provide the maximum required quality at a cost competitive with other construction materials. Failure to recognize the limitations of the relationship between W/C ratio and strength and the other relationships, examples of which were mentioned previously, can result in concrete which does not provide the required properties on the one hand or which either prices concrete out of the competitive market or makes wasteful use of our resources on the other. Concrete production has become a science instead of an art and it should be produced on a scientific basis. Professor Gilkey has done the industry a great service in calling our attention to the desirability of taking another look at the W/C ratio versus strength pronouncement. Let us all be sure that we take this look and also that we take another look at all of the aspects of concrete design to verify that we are not considering general relationships as law rather than useful guides, and that their validity as applied to a specific job has been demonstrated by optimum performance with the particular job materials and job conditions.

By J. HOLLEB*

Being an heretic of the "water-cement ratio law," the paper by Professor Herbert J. Gilkey was to me a big comfort and encouragement, and I am grateful to him. It was an agreeable surprise to find in his extensive and largely documented study, a confirmation to the results of my own experiments and thinking.

It needed some courage to be opposed to a theory accepted by the conventional wisdom for many years, as a dogma, a method recommended by the distinguished Portland Cement Association, but such a scientific theory must respond to two postulates to be accepted. First, the reasoning to support the theory must be logical. Second, the theory must be confirmed by experiment. Whenever any of these conditions are unsatisfactory, the theory is wrong, and we have to re-think the problem and make new experiments.

It is not the writer's intention to propose to change a method considered by many as a sound engineering practice (but not by all, e.g., the Illinois Division of Highways has adopted the "mortar-voids method"

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by Talbot and Richart. The writer would like to remark that he is not a follower of this method) and solve easily all the problems of proportioning. His intention is not to propose a new theory but only to express some thoughts and facts to stimulate serious thinking on these problems and to make new scientific investigations and experiments.

To compare the strength of concretes, we often forget to take in account the quantity of cement used per unit volume or per unit weight. By a rational proportioning method, the strength of concrete (within certain limits) is proportional to the amount of cement used. As we increase the amount of cement (per unit volume of concrete) we must also increase the amount of water. For a given mixture, it is necessary to add water and cement to make the concrete stronger. Adding 20 percent, approximately, of water per 100 percent of cement, the W/C ratio is lower. This is arithmetic. The more cement is used, the lower the W/C ratio and the stronger the concrete is, but for different mixtures using the same amount of cement with different maximum sizes of aggregates, gradings, and proportions with the same W/C ratio, the workability of the fresh concrete will be variable and the strength also. If we adjust the quantity of water (for the same amount of cement per unit volume) to have the same consistency, the strengths would be variable and evidently, the W/C ratio will also be variable.

The problem of proportioning is to establish with a given amount of cement a mixture giving a workable concrete with a minimum amount of sand and consequently, a lower W/C ratio and maximum strength. The W/C ratio cannot be fixed a priori. This would be like putting the plow before the oxen.

The magic power of the W/C ratio law is that the W/C ratio is not related to the amount of cement used per unit volume and is limited only to plastic mixtures. This, in the writer's opinion, distorts the problem of proportioning and guides us to a poor engineering practice. Mixtures with different maximum sizes of aggregates, gradings, and proportions as well as materials, would have different plasticities, but it is possible by adding water and cement to have the same plasticity, the same W/C ratio, and approximately the same strengths only with variable amounts of cement used per unit volume.

In practice, the danger is to add sand and water to secure a plastic mix but not adding cement at the same time will not produce the prescribed W/C ratio.

In theory, it does not answer logically the question as to what really makes a workable concrete of a maximum strength with a given amount of cement. A factor which greatly influences the strength of concrete and which is overlooked is the energy spent for its compaction (without segregation). The more energy spent, the less plastic is the concrete and the stronger it will be.

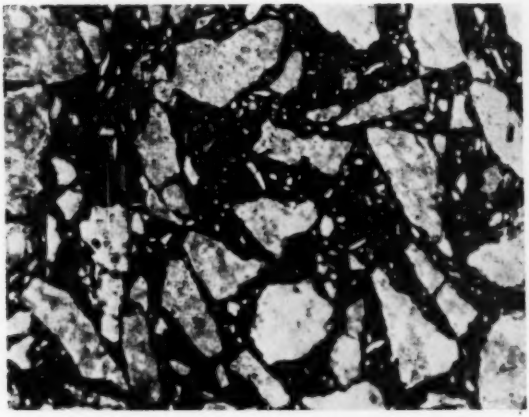

Fig. A — Type A concrete

We should define as workable a concrete mixture which with the given compaction method gives an homogeneous concrete (without segregation) and without voids. It is a relation between the energy used for compaction and the strength of concrete. The laboratory experiments show that a concrete compacted energetically with a heavy tamping plate is equal to a vibrated concrete. Evidently, a good manual compaction in the field is not realizable and the use of vibration has to be generalized. The W/C ratio method, which restricts its application to plastic mixtures which are normally oversanded, cannot be vibrated because the mixtures will segregate and restrain further progress in the technology of concrete. Obviously, if the economy in cement is not important and quality is not required, the W/C ratio method gives an easy solution to the problems of proportioning.

The writer agrees with Professor Gilkey's statement that the strength of the concrete is not the strength of its mortar. Referring to Table 1, p. 1303, in the comparison of rich and lean concretes and their mortars, Professor Gilkey concluded that the strength of the mortars is higher than the strength of the concretes. Here, the writer disagrees. To compare the strengths, we relate them to the amount of cement used per unit volume or unit weight. Considering this, the concretes in this table are stronger than the mortars. In good proportioning, the strength of the concrete is higher than its mortar, and in poor proportioning, it can be equal or lower.

The art of proportioning is to make the coarse aggregate play an active role in increasing the strength of concrete. To consider the coarse aggregate as an inert filler is degrading, and we have to rehabilitate its dignity.

The maximum size of the aggregates, the grading, and their proportions, may vary the amount of mortar required to have a workable

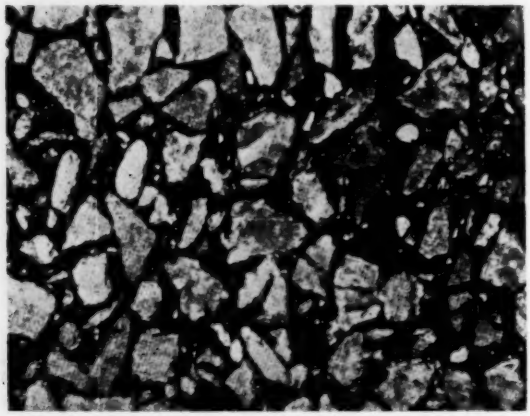

Fig. B — Type B concrete

concrete. Consequently, it is evident that the lower the sand-cement ratio, the stronger the mortar and the concrete will be.

It's a fallacy of the Talbot-Richart method to determine the strength of the concrete by a presumption after getting the strength of the mortar. This is a second case where the plow is put before the oxen.

It can be concluded that the first problem to be solved is the size, grading, and proportions of the aggregates. If the importance of this problem is not clarified and explained, the results of new experiments will remain confused. The writer would like to give the following example to illustrate his point:

A coarse sea or lake sand is considered to have a poor grading and forms a weak mortar but the same sand used with aggregates of adequate grading and proportions produces a strong and workable concrete. Trying to understand and explain these confused facts, the writer made this experiment: Hundreds of samples of concrete presented in the laboratory for tests (having different compositions, sizes, gradings, and proportions) were sawed to compare their structures. It was apparent that the strongest and most homogeneous concretes had two typical structures:

Type A—The coarse aggregates are mostly of the same size, embodied with cement mortar. Between them are aggregates with smaller sizes but larger in number, also embodied in mortar. The voids between the coarse aggregates are filled only with these small embodied aggregates in mortar, and not with mortar (see Fig. A).

Type B—The coarse aggregates are more variable and closer to each other, and the smaller coarse aggregates are finer. These strong and homogeneous concretes were obtained by a discontinuous grading of the aggregates (see Fig. B).

If it is necessary, the writer will try to expose in an additional paper how the maximum size, the grading, and the proportions of aggregates are so essential to form the characteristic structures.

By L. LIBERTHSON*

This paper deserves to be ranked as one of the most notable contributions of the subject in many years. Indeed, the author should be commended because such examples of basic inquiry, into what has come to be regarded as an almost immutable law, have been so rare in the past as to be almost inconspicuous.

The first statement of Professor Gilkey's summary may generate a question in more than one reader's mind, to wit: what and how many pronouncements relating to other than the W/C parameter, but emanating from nontechnical and professionally unqualified sources, belong in the category of "serious oversimplifications that block sound thinking?"

The most glaring example that comes to this writer's mind involves admixtures, and this is not to be construed as a reflection on admixtures per se but only on certain prevalent practices in the promotion of admixtures. To quote, the pronouncements:

> . . . that this or that admixture can make concrete "100 percent impermeable;" or that this or that admixture can "water proof" concrete; and, to crown the pronouncement,
>
> . . . that when this or that admixture is used in a design mix, the concrete is "guaranteed" to be indeed impermeable and water proof, usually for an arbitrary period of say five years.

This kind of oversimplification becomes serious because it is too often supported by manipulated evidence by way of test results. The design engineer then discovers that the net actual function of the particular admixture is to introduce into the mixture a pound of calcium chloride per sack of cement.

It is probably unnecessary to drag economics into the discussion. Calcium chloride is an inexpensive, seasonally useful, desirable, if not mandatory, adjunct in normal concrete practice (note, we say adjunct not admixture). But calcium chloride is bottled, canned, and drummed (figuratively as well as literally) in the form of an aqueous solution, and it assumes a level of omnipotence which even the manufacturers of calcium chloride have never dared to ascribe. Should the claim be made in defense of such practice that the use of such an admixture improves the quality of the concrete it is only necessary to ask the entrepreneur: "Well, just *how* does it do so?" Should the dubious answer emerge that it permits reduction of the W/C ratio the engineer

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is on at least safer ground. But how much safer? This leads directly to Professor Gilkey's analytical reappraisal of Abrams law.

The writer maintains that there exist some extremely pertinent data from such sources as the U.S. Bureau of Standards and the National Ready-Mixed Concrete Association, which completely contradict the oversimplification that calcium chloride could make concrete 100 percent impermeable.

The writer hopes that Professor Gilkey's contribution will inspire one of us to discuss this problem more completely.

By INGE LYSE*

Professor Gilkey should be highly commended for once more stating so clearly that the W/C ratio does not tell the whole story of the quality-giving properties of concrete. Everyone who has experimented with concretes of various compositions and of different materials will no doubt fully agree with the statement that many factors besides the W/C ratio have considerable influence on the strength of the concrete. The main difference is probably in the way of expressing one's viewpoint. For given materials under given conditions such as one finds on a specific construction job the W/C ratio gives an excellent basis for evaluating the strengths. But for different materials (differences in type, size, shape, grading, surface characteristics, etc.) we are all aware of the fact that the influence of the materials of concrete is significant. Various types and makes of the cement will give widely different strengths for the same W/C ratio. Investigations of different types of ordinary aggregates from one district gave large differences in strength for the same W/C, in fact as much as 50 percent,[27] which was about the maximum difference found for 13 different standard portland cements from Lehigh Valley[28] for equal W/C ratios. If one considers the results plotted in Fig. 1 it is seen that Abrams results also gave as much as 50 percent maximum difference for a given W/C ratio.

Even for the same materials the results may vary widely because of variations in water content and consistency of the mix and the amount, size, and grading of the aggregates.

By careful analysis of the manufacturing, curing, and testing of the specimens of concretes containing the same materials it is often possible to account for much of the differences in the results. Bleeding of wet mixes, particularly lean concretes, mortars, and neat cement paste of high fluidity will cause a considerable reduction in the actual water content in the hardened mass and produce laitance in the specimens. The writer studied the problem mentioned in Reference 19. The lack of effect of size of the test specimen was apparent in this investi-

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gation. But the bleeding or separation varied considerably and by careful observation of the amount of water separated from the concrete, corrections could be applied which agreed reasonably with the various mixes. In the mixes given in Table 1 there must have been large differences in the amounts of water which separated from the mortars and the concrete. Therefore considerable differences would be expected in the strength results for a given W/C ratio. The McMillan suggestion[7] for testing concrete with large-sized aggregates was a valuable practical field method. Even today it is recommended on certain construction jobs.

The bond between the cement paste and the aggregate particles will naturally influence the strength of the concrete. Gravel and crushed materials will generally give different strengths. Smooth surface materials will generally give lower results than rough surface materials; nonabsorptive materials lower results than absorptive materials; large-sized smooth aggregates may give lower strength than small aggregates because of lower bond values, etc.

But all this does not change the fact that the W/C or C/W ratio is an excellent guide for the strength relationship for concretes having given materials under given conditions. The writer considers McMillan's excellent book; *Basic Principles of Concrete Making*,[21] as presenting the most outstanding practical explanation of the composition of the concrete and of the quality-giving properties of the concrete. This book has more than any other publication brought the W/C ratio to the place it has today among concrete engineers throughout the world.

The author should be given high recognition for his everlasting insistence on the many factors which in addition to the W/C ratio determine the strength of the concrete and for the clear manner in which these viewpoints were presented.

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28. Lyse, Inge, "Effect of Brand and Type of Cement on Strength and Durability of Concrete," ACI JOURNAL, *Proceedings* V. 31, 1935, pp. 247-271.

By M. SPINDEL*

This paper, like the previous one by Walker and Bloem[10] is a fully entitled challenge to the idea that the W/C ratio alone is sufficient to indicate the strength to be obtained in mortar and concrete. As the writer already pointed out† there are many reasons why the W/C ratio alone is not sufficient to indicate the strength of mortar and concrete.

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†Spindel, M., Discussion of "Effects of Aggregate Size on Properties of Concrete," ACI JOURNAL, *Proceedings* V. 57, No. 9, Mar. 1961, p. 1231.

Many of these reasons were given more distinctly by the author, but some supplements may still be warranted.

The writer, formerly head of the Materials Research Station of State Railways, member of the International Association for Testing Materials and member of various committees dealing not only with cement and concrete but also with materials of every description, fully understands the feelings of the author with regard to the importance of size, shape, surface texture, and distribution of particles in mortar and concrete, as, he pointed out, is usual for metals.

It is true that both in different types of metals and natural stones the materials with the smallest particles have, as a rule, the highest strength and durability, but it is not quite the same with concrete. With regard to the latter the writer fully agrees with the author's statement that the long-standing concept of the aggregate function as being solely that of an inert filler cannot be accepted.

To this it may be added that the writer always considered the aggregates to be the most important part of strength and durability of concrete. First of all it is not sufficient that the aggregate should just have the strength of the hardened cement, but the higher the strength of the aggregate, the higher the strength of concrete that could be obtained if size, shape, grading, and surface texture of the aggregate are the best suitable for the concrete in question.

The quality of cement is, of course, important, but the cement paste has to be considered only as a type of glue which must not be too much diluted by water and be just enough to fill all the pores in sand and coarse aggregate completely.

The only particles not stressed in the paper were the voids, which play an important role in mortar and concrete as in most materials, inorganic or organic. In the formula by Feret mentioned by the author and in a similar formula by Talbot and Richart, the quantity of voids is already in their formulas but, unfortunately, not so in the formula by Abrams. In this concrete is considered to be free of voids, or otherwise the formula certainly would not be valid.

With regard to the many uncertainties in the tests carried out with coarse aggregate size according to standards the writer wishes to stress some additional points in the hope that the author or some of the readers concerned might be willing to help using them satisfactorily.

Owing to the fact that portland cement consists mainly of four different chemicals, viz., CaO, SiO_2, Al_2O_3, and Fe_2O_3; and further that mortar and concrete consist of four different substances, viz., cement, aggregates, water, and air, the writer succeeded in putting these four matters graphically into a diagram known as the "four matter parallelogram by Spindel." It was published first in the 50 Years Jubilee Num-

ber of the *Tonindustrie Zeitung* (Berlin), 1927, and afterwards further developed in leading periodicals and international congresses.

This four matter parallelogram for cement was soon accepted by leading scientists on cement, such as Professor Kuhl in Berlin and other cement specialists in Europe and even in Japan. Although this diagram could easily be handled matter and though it was a quite correct projection of four matters in space, it was not generally used. Even when the writer succeeded later in expanding this four matter parallelogram to a five matter polygon in which not only the four main constituents of cement, but also their hydrates could easily be shown and their percentages directly read from the graph up to one-tenth of a percent, it did not help introducing it.

Much more advantageous than when using it for determining the best composition of cements, this four matter parallelogram offered a great help to the designers of the various types of concrete. To every point in the diagram showing the absolute volumes of cement, aggregates, water, and air the strength of mortar and concrete could immediately be read from a curve on the top of the diagram showing the strength for each known strength of the cement used according to the formula by Feret.

With a small supplement to this diagram even the volume or weight of the concrete in question could be shown in the diagram without any calculation. Into this four matter parallelogram even the fineness modulus by Abrams could be shown and read easily.

When working with entrained air, the necessary percentage of air had to be taken first and the volume percentages of the other parts could be shown and read easily without any special calculations, always giving a full 100 percent.

The writer fully understands that chemists for a long time accustomed to the triangle hesistate to change instead of manipulating with four matters in triangles, to use the parallelogram which no doubt is much easier to handle than the triangle even for three matters only.

The difficulty in the United States in using the parallelogram may be that most publications in Europe are based on the decimal system. The conversion from the decimal system, without showing the percent, of course, can easily be done, as in Fig. 6A, 6B, 7, and 8 of the *Concrete Primer*,* but the writer would gladly supply the reprints of the European publications if requested.

The writer would like to add one more point with regard to making and testing the quality of concrete. As a member of the committee of the International Association for Test Materials dealing with the resistance to frost of natural stones, the writer agreed with the suggestions for microscopic examination and special testing methods for the water

*McMillan, F. R., *Concrete Primer*, ACI Publication, 1958, Detroit, Mich.

absorption coefficient made at the Congress of International Association for Testing Materials. These suggestions were not accepted at the New York Congress in 1912. In the writer's opinion it would be appropriate to take up this matter again after 50 years of testing concrete, which at present is often much stronger and better than natural stones. In the writer's opinion the author has the required reputation to set off the necessary impetus in the ACI JOURNAL.

As a summary of the present and the previous discussion mentioned at the beginning, we may conclude that

1. Everybody concerned has to be grateful for the explanations given in this paper by the author which will contribute much to improve the usual methods of proportioning concrete based on the W/C ratio.

2. Whether or not air entraining is used there are still voids left in the concrete which reduce the strength, not only in proportion to their quantity, but also depending on the size, shape, and distribution of air pores. Therefore, special methods of microscopic tests will have to be used.

3. Whatever testing methods are used, the slump test is not sufficient to give satisfactory results regarding workability of concrete. Therefore, more modern methods such as vibrating and recording the setting will certainly give more reliable workablity results and the necessary W/C ratio for the concrete in question.

4. The latest development in improving the high quality of concrete, and to obtain certain special advantages for different types of work, is by adding suitable admixtures of different composition and different effect. These, too, must not be overlooked. They are used in concrete just as special additives are used in the manufacture of steels and metals, although even here certain chemicals cannot be traced later on even by chemical analysis.

The writer would now like to point out what has been achieved with successful research even before the theoretical background was fully clarified regarding the composition of burned cement clinker. Using the most suitable composition of raw materials found in his research with burning in an electric furnace, the writer obtained the first high-early-strength portland cement in his research laboratory of the State Railways back in 1912, and then manufactured this high quality cement in 1913.

With the results of the most suitable grading obtained before by his research, the writer succeeded in obtaining the concrete strength standardized for 28 days within 2 days of hardening. These results were soon made use of in constructing concrete bridges for the railways. The first bridge of reinforced concrete and the second of plain concrete which were loaded after 2 days of hardening were made under

supervision of the writer in 1915 and 1916 and the results were immediately published* and accepted as great progress.

Although the strengths of concrete and cement obtained then have so far not been surpassed, it is important to note that the high strength qualities of concrete were more fully appreciated only much later on with the introduction of prestressed concrete. The years 1962-63 will be the 50 jubilee year of this first high quality portland cement and concrete.

By STANTON WALKER and DELMAR L. BLOEM†

Professor Gilkey's re-examination of the *W/C* ratio strength relationship is most timely. It represents a valuable contribution to concrete technology. There has been too little understanding of the limitations on the applicability of the water-strength relationship. Specifications, generally, consider *W/C* ratio as fixing strength, in spite of the widely known fact that other materials also affect the strength level. Even that the strength-producing properties of cements differ is overlooked more often than not.

These writers are appreciative of the generous recognition given their papers on the effect of maximum size of aggregates.[9,10] As pointed out in their ACI paper, it was Professor Gilkey who caused them to study the record and uncover much early data in support of what they had considered a new development. In spite of those early data, the writers wish to add something to Professor Gilkey's chapter on "Extenuation."

A principal feature of the size-strength relationship is that smaller sizes produce higher strengths than larger sizes for a given *W/C* ratio. This appears always to be true for concrete strengths within the range now generally used. Further, for a given cement content and concrete consistency, more mixing water is required for the smaller than for the larger sizes but the differences are much greater for lean mixes than for rich ones. For these reasons, the beneficial effects of smaller sizes are obscured, or even disappear, in low strength concrete and are emphasized in high strength concrete. The strength level above which concrete of the same cement factor and consistency shows the smaller sizes to have greater strength was found in the writers' tests to be, approximately, of the order of 3500 to 4000 psi.

In the early investigations by Abrams, strengths in excess of about 4000 psi were rare for the usual concrete. Mixing water in the amount of 7½ gal per sack of cement gave 2000 psi instead of 3500 to 5000 psi. Accordingly, the effects of size were not so readily discernible in the earlier investigations.

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*Spindel, M., "The Use of High Quality Special Portland Cement and Concrete in Structures," *Wochenschrift für den öffenlichen Baudienst* (Vienna), No. 22 and 23, 1916.

†Members American Concrete Institute, Director of Engineering, and Associate Director of Engineering, respectively, National Sand and Gravel Association and National Ready Mixed Concrete Association, Washington, D.C.

Professor Gilkey's restatement of the *W/C* ratio law is good. However, the writers would modify it somewhat as follows: "For a given combination of cement and aggregates, and for the same conditions of mixing, curing, and testing, the strength of concrete is determined by the quantity of mixing water. For a given *W/C* ratio, the strength of concrete is influenced by a number of factors, including principally: (1) strength-producing properties of the cement; (2) strength and surface texture of the aggregate, particularly the coarse aggregate; (3) size and grading of the aggregate, particularly the coarse aggregate; and (4) the ratio of cement to aggregate." It appears to the writers that this is to say that strength of the concrete is the resultant of: (1) strength of the cement paste; (2) bond between the cement paste and the aggregate; and (3) strength of the aggregate particle. The strength of the cement paste predominates but is modified by bond and aggregate particle strength.

The fine portion of aggregate is composed of a large number of particles and, consequently, has such a large surface and cross-sectional area that problems of bond and aggregate strength should not loom importantly. The writers suggest, therefore, that a somewhat more understandable, even though perhaps less accurate, statement is that the strength of concrete results from: (1) the strength of the mortar; (2) the bond between the mortar and the coarse aggregate; and (3) the strength of the coarse aggregate particle, i.e., its ability to resist the stresses applied to it. The writers made essentially this statement in the closure to the discussion of their ACI paper.*

By G. B. WELCH†

It is particularly pleasing to see the *W/C* ratio strength law questioned, as its exact application in a number of circumstances has certainly been doubtful for some years. In addition to the effects on strength of aggregate properties as shape, surface texture, and elastic modulus, as mentioned by the author, the effect of the aggregate-cement ratio is considered to be of great interest and has been the subject of investigations by the writer at the Cement and Concrete Laboratories of the University of New South Wales.

Several investigators in the United States, as quoted by the author, have indicated that a reduction of the aggregate-cement ratio results in increased compressive strength. On the other hand, a number of investigators in the United Kingdom have shown the reverse effect. The effect of reduced aggregate-cement ratio causing reduced strength with high strength concretes was pointed out by McIntosh of the British

*Walker, Stanton, and Bloem, Delmar L., Closure of "Effects of Aggregate Size on Properties of Concrete," ACI JOURNAL, *Proceedings* V. 57, No. 9, Mar. 1961, p. 1248.

†Member American Concrete Institute, Senior Lecturer and Head, Cement and Concrete Section, University of New South Wales, Australia.

TABLE A—TYPICAL SERIES OF CONCRETE MIXES

Aggregate-cement ratio	W/C ratio	Sand, percent	Slump, in.	V.B. deg	Compaction method	7 day compressive strength, psi	Standard deviation, psi
5.0	0.40	25	0	17.5	Table vibration, 2 min	5830	125
4.3	0.40	23	1½	4.8	Table vibration, 1 min	5450	410
3.8	0.40	22	2½	4.0	Hand tamp	5150	245
3.3	0.40	21	3½	3.0	Hand tamp	4870	226
3.3*	0.40	21	3¼	3.2	Hand tamp	4950	138

*Remixed after 1 hr.

Cement and Concrete Association, in 1952,[29] who also suggested that, for given materials, the strength was inversely related to the workability (as measured by the British compacting factor test). Other investigators who subsequently supported these findings included Erntroy and Shacklock,[30] and Stewart.[31] Erntroy and Shacklock suggested that a decrease of 1.0 in the aggregate-cement ratio had the same effect on strength as an increase of 0.02 (by weight) in the *W/C* ratio; Stewart indicated a similar trend with concretes of *W/C* ratios ranging from 0.35 to 0.60.

The writer has examined this question in connection with an investigation into methods of high strength mix proportioning,[32] and has found significant strength increases with leaner mixes, using standard cylinder specimens (rather than cube specimens as used in the United Kingdom tests). Concrete mixes ranged from low workability requiring intense vibration by a table vibrator to compact, to rich extremely cohesive mixes of about 4-in. slump.

Results of a typical series of concrete mixes, using irregular river gravel and river sand (saturated, surface-dry), with high-early-strength portland cement, are shown in Table A.

A simple explanation for the effect of the increased aggregate-cement ratio on strength involves the reduction of cement and water contents, per unit volume, with leaner mixes and consequently the reduction of excess water voids per unit volume of concrete. An additional explanation which seems logical involves reduced settlement and water-gain, and their influence on the aggregate-matrix bond. This influence has been demonstrated by a series of tests by the writer on cubes tested on their side and vertically as cast (after capping); cubes tested vertically gave an average apparent compressive strength 15 percent greater than when tested as normally on their side. Although insufficient experimental evidence is available, it seems likely that the apparent increase in strength would be less marked with leaner mixes.

The effect on compressive strength of excess water voids, and the corresponding change in density, does not appear sufficient to explain

the considerable influence of variation in aggregate-cement ratios. Also, the water-gain phenomenon, while undoubtedly playing a part in affecting the strengths of mixes with high W/C cement ratios, would seem to be less important with concretes in the high strength range.

The reasons for the different strength results with variation in the aggregate-cement ratios are of course complex and not fully understood. One important factor involved appears to be the packing of coarse aggregates; a packing arrangement approaching point to point contact is less dependent on the relatively weak tensile bond strengths.[33] The increased strengths reported with extremely lean, gap-graded concretes (see discussion of Reference 31) may be influenced by this factor.

Other factors involved are the bond strengths of the cement paste and mortar, which are in turn influenced by paste sedimentation characteristics, richness of mortar, and aggregate surface characteristics. The constitution of the mortar also affects the concrete properties, and it is possibly here that differences occur between the higher strength concretes investigated in the United Kingdom and those reported by the author. The stiffness of the mortar should be increased by better fine aggregate packing and also by leaner mixes, as the excess water voids are reduced (as previously mentioned). Stiffer mortar should reduce settlement effects but on the other hand richer mortars might be expected to increase bond strengths.

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By WALTER H. WHEELER*

Professor Gilkey included two statements in his paper with which the writer fully agrees.

1. Doubtless entirely unrecognized at the time was the fact that what was hailed and believed to be a revolutionary change in basic concrete

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technology was actually nothing more than an operational improvement; a manipulatory refinement in technique.

2. At equal values of water-cement ratio the strengths of workable concrete mixtures, fabricated, cured, and tested under similar conditions (in equal-size cylinders of appropriate diameter), may be influenced greatly by such factors as: The grading and relative amounts of the aggregate particles.

Item 2 is quoted from the report of Walker and Bloem on tests they had made. When the *W/C* ratio concept was first brought to the writer's attention at a meeting of the N. W. Section of the American Society of Civil Engineers, the writer asked the speaker: "What about the affect of the aggregate on the strength of the concrete?" In reply the speaker stated in effect that the aggregate was immaterial; it could be brick bats or almost anything. With that the writer sharply disagreed and has not had occasion to change his position. While he respects the value of the *W/C* ratio as a guide the writer has never accepted it as the only criterion for making good concrete. From his first experience in making concrete as a construction engineer on the large coal and coke plant at Dawson, N.M., for a subsidiary of Phelps Dodge Co., about 56 years ago, it was the writer's practice to use the minimum amount of water consistent with good workability and to select sound well graded aggregate with good bonding properties. It happened that another engineer who was in charge of the construction of the coal washer for which he designed reinforced concrete walls, foundations and floor slabs, and who later became the writer's partner in an engineering and contracting business in Denver, was well versed in concrete technology. The writer would like to give three examples in which the aggregate was proved to be an important factor in the success of the project.

In January 1908, E. Horton Jones and the writer formed a partnership and engaged in private enterprise in Denver, Colo. The first job was to find the solution to the troubles which the Colorado Fuel and Iron Co. was having with the heavy stone masonry walls of their banks of beehive coke ovens. The space around and over the top of the beehives was filled with earth. As the ovens heated up the backs of the walls expanded more than the fronts and hence the wall moved forward at the top slightly. The earth fill over the oven became red hot and flowed like water and filled the space between the oven and the wall. When the oven cooled after the coke was pulled the wall could not come back. Result, in a year or two the walls would fall forward and have to be rebuilt. After a rather thorough study of the matter including measurements of temperatures on the backs of the walls (over 1000 F) the writer decided that a thin reinforced concrete cantilever T-section wall could be designed, using the proper kind of aggregate, which would stand up and take it. The writer's company was commissioned to construct a section of wall that would extend over the

length of three ovens, and did. Aggregate was selected that would take the alternate heating and cooling and that was carefully graded to ¾-in. and under. The top of the wall moved out the calculated distance due to the high heat on the back and remained there for the 6 years the ovens were in operation. As a result the writer's company built more walls. The Denver company abandoned beehive ovens in favor of Koppers ovens at the end of 6 years, and the writer removed the earth fill and examined the back of one of the walls. The surface of the concrete was calcined for about ¼ in. in depth. The calcined surface protected the rest of the concrete in the wall and also the reinforcement bars. Otherwise the walls showed no change, no cracks, and apparently could have been used for many more years.

Jones and Wheeler became Jones-Wheeler-Cramer Engineering Co. and in the fall of 1908 the company started construction of the reinforced framework of the Century Store and office building, 75 x 125 ft, four stories and basement with columns and footings designed to carry four additional stories in the future. The framework was designed using the Turner Mushroom System of flat slab construction. This was the first skeleton frame reinforced concrete building to be constructed in the mountain states and one of the early flat slab buildings. The sand and gravel were stockpiled from the excavation for the basement. It was clean and excellent concrete material but deficient in proportion of coarse aggregate. The aggregate was screened to remove oversize stone (over ¾ in.). Slag, which was obtained from the dump of a former custom smelter, was screened to eliminate fines and oversize. Enough slag was added to the mix to build up the coarse aggregate to about a 1:2:4 mix, with minimum water. The city building department refused to give a permit for the building but allowed construction subject to a fire, load, and water test to be made on the first floor slab after the frame was completed. This test consisted of 200 lb per sq ft of pig iron spread over one bay. While the load was on the floor a large wood fire was built in the basement with the flame striking the bottom of the slab. When the slab was considered to be red hot, a fire hose was turned against the under side of the heated slab. The bay tested measured 15 x 20 ft center to center of columns. The deflection before the fire was 3/16 in. and after the fire to the amazement of the building inspector it was ⅛ in. There was no spalling of the concrete where the fire had been. In 1920, the writer made the structural drawings to incorporate the Century Building into the 10 story U.S. National Bank Building. This involved extensive alterations in the structure of the Century Building. Wherever concrete was cut it was found to be excellent.

In 1923 the writer was commissioned to locate, design, and supervise the construction of the Fort Snelling Mendota Bridge and connecting

highways. The bridge crosses the Minnesota River and its ¾ mile wide valley. The writer quotes from his article in *Engineering News Record*, Mar. 31, 1927:

> A minimum mix of 1:2:4, using gravel or crushed rock was specified. Control of water ratio and a minimum strength of 2400 psi on concrete test cylinders at the age of 28 days were also specified. The sizing and quality of aggregates were particularly specified, including hardness, toughness and water absorption for the coarse aggregate, thus eliminating materials known to disintegrate rapidly by frost action. · · · Sand inundators were installed in 1925, although good and uniform concrete had been secured previously by careful handling. The total amount of water in the concrete per sack of cement ranged from 6½ to 6¾ gal. This was reduced to 6¼ gal after the inundators were installed without reducing workability and a more uniform consistency was obtained. The strength of concrete test cylinders in compression ranged from 2413 to 2825 psi before inundators were installed. Those taken with the inundators in use ranged from 2672 to 3521 psi.

During World War II a hole was cut through the deck slab by first coring around the hole. These cores were tested by the Minnesota State Highway Department and averaged about 6600 psi. The concrete in the bridge is still in excellent condition.

By HUBERT WOODS*

The *W/C* ratio law is an empirical one based, as are all empirical relationships, on observation rather than on basic theory. Such a relationship has the merit of coordinating a large number of observations, of putting them into conveniently useful form. It has the disadvantage of not being universally applicable, and indeed may be quite incorrect under circumstances sufficiently remote from those under which it was derived. The originator of a useful empirical relationship is often inclined to overstate its area of applicability, he being so engrossed in the work which led to it as not to be entirely conscious of unexamined conditions which do in fact circumscribe and limit it.

There is understandably a tendency on the part of those using an empirical relationship to attempt its extension to different circumstances. Sometimes this extension may involve only a change of constants in a mathematical expression, the general form remaining the same. This is quite permissible if the result is useful, and it has been useful in the case of the *W/C* ratio expression. Some serious investigators think of the *W/C* ratio primarily as an indicator of the quality of the paste in concrete, and indeed this view has much to commend it.

Professor Gilkey has documented his useful and interesting paper with references to some of the more significant early contributions to his topic. The writer would like to add a few more. Although he men-

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tions the work of Feret as preceding that of Abrams, he did not give a specific reference. That work was reported[34] while Feret was chief of the Laboratory of Roads and Bridges at Boulogne-sur-Mer. To Feret must be given historically the credit for first relating strength to the absolute volumes of ingredients of the mix. He said, "For any series of plastic mortars made with the same binding material and inert sands, the resistance to compression after the same length of set under identical conditions, is solely a function of the ratio

$$\frac{c}{w+v} \text{ or } \frac{c}{1-(c+s)}$$

whatever be the nature and size of the sand and the proportions of the elements — cement, inert sand, and water — of which each is composed." In these expressions, c, w, s, and v are the absolute volumes of cement, water, sand, and air voids. Feret derived the equation $P = K[c/(1-s)]^2$, where P is compressive strength and K is a constant depending on the system of units employed, on the cementing medium, and on the curing time and conditions. It is of interest to note that Feret took cognizance of air voids.

Another early work on the W/C ratio which seems to have escaped much attention in the United States is that of Zielinski in Budapest.[35] It was called to the writer's attention some years ago by Niels Plum of Denmark. Apparently the Hungarian work was done without knowledge of Feret's earlier work. The Hungarian report shows diagrams of strength plotted against W/C ratio (the individual curves are called "water lines").

Although the two works just mentioned presented W/C ratio laws, there was even earlier recognition, with some disagreement, that high water contents resulted in low strength. L. J. Vicat in about 1856 is said to have concluded that "a large dose of water invariably injures cement mortar."[36] Several other well-known authorities did not agree. Gillmore[36] himself pointed out that Vicat's conclusion was based on tests with natural cement, and seemed to imply that this conclusion might not hold for portland cement.

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AUTHOR'S CLOSURE

The favorable reception accorded to the paper, considered in conjunction with the interest created by the recently published evidence of Walker and Bloem,[8,9,10] lend encouragement to the hope that at last the *W/C* ratio relationship can, in our thinking, be divested of its long-held all-embracing, near-deified status to become recognized for what it is: *one of the several* major factors that influence the strength and some of the other important properties of concrete. The variety, quality, and nature of the discussions are appreciated and should add materially to the clarity and effectiveness of the message the paper was designed to deliver.

Mr. Burmeister's recital of variability "within the law" is typical and points up the difficulties regularly encountered by persons operating over a wide range of conditions where both the cement and the aggregates vary from job to job. Mr. Burmeister's broad range of materials, mixtures, and applications made his problems especially difficult and nonconformable to the lone *W/C* generalization — too many other unevaluated but significant variables were also present.

Mr. Cook's discussion of the proposed rewording of the *W/C* versus strength pronouncement (Item 5, p. 1309) is to the point. As indicated later in connection with the Walker-Bloem discussion, and at the end of this closure, the author believes that Item 5(c) should be reworded to include both shape of particle and strength of the aggregate as essential properties. He does not for reasons stated, propose deletion of stiffness or the addition of hardness.

Stiffness, as a synonym for rigidity measures resistance to deformation. Adequate stiffness, along with strength (resistance to fracture or disruption), probably constitutes the equivalent of what Mr. Cook has in mind for hardness. From the standpoint of definition hardness is an elusive term — one difficult to pinpoint. As Mr. Cook knows, two types of hardness are commonly recognized: (1) resistance to penetration and (2) resistance to abrasion. Among the varied tests proposed and used for measuring hardness the results of such a test as that made by the widely-used scleroscope, for example, is unquestionably influenced by still other characteristics of the material. The term stiffness is definite in its connotation. Stiffness represents resistance to general deformation (not localized deformation such as that represented by penetration or wear). One might mention, in passing, that the so-called modulus of elasticity (literally "measure of elasticity") is a misnomer in that it doesn't measure elasticity — it is actually the measure of stiffness or rigidity, (the modulus of rigidity).

The author likes Mr. Cook's characterization of concrete as, "the most versatile of all construction materials." He too once attempted, somewhat more volubly, to give expression to the same thought: "As a

structural material concrete owes its important place to the fact that, properly controlled, it is economically utilitarian, highly resistant to fire, wind, water, earthquake, and war, and lends itself readily to almost any type of decorative or architectural expression, ranging from the conservative to the fantastic." Obviously we are here concerned with the words "properly controlled" to which aspect Mr. Cook's timely comment is directed.

Mr. Holleb, while placing himself definitely in the ranks of the *W/C* dissenters, disagrees with the author's statement that the strength of a concrete is less than the strength of "its" mortar. Unless the author fails to understand Mr. Holleb's statement there is no actual disagreement.

At equal cement factors and the same *W/C* ratios the strengths of concretes greatly exceed the strengths of mortars, and this is evidently what Mr. Holleb has in mind. By "its" mortar the author means the mortar that would be secured by wet screening the concrete of which the mortar is part. This would be "it's" mortar but would obviously have a cement factor greatly in excess of that of the concrete from which it was screened.

Mr. Liberthson's comments on admixtures reminds the author of a definition he once heard given for an admixture for concrete: "a by-product for which no other use could be found." While this, like the *W/C* pronouncement itself, constitutes an over-generalization it points up the fact that claims made for admixtures, like those for competitively advertised products in general, should always be subject to the type of scrutiny exemplified by the engineer's motto: "In God we trust — after having been duly checked."

The author feels that Dr. Lyse has, in his opening paragraph supplied an excellent summary; one in which the aggregate is no longer relegated to the role of inert filler, devoid of promoting slippage, splitting and others of the stress-raiser manifestations of heterogeneity.

Only in the carefully phrased third paragraph does there appear to be a touch of the old loyalty to a *W/C* pronouncement formerly accepted as absolute and inviolate.

Admittedly the visible water separation that accompanies the plastic sedimentation phase common to all freshly placed concrete, and especially to the more fluid or high-slump mixtures, is a disturbing, provocative aspect about which a book or two could be written (if we but knew what to write). At first thought the inclination to try to estimate or measure the extent of water separation and to compute therefrom a "corrected" value for *W/C* ratio appears to be commendably analytical and sound.

One of the several points that has seemed invariably to have escaped notice in this approach is the fact that all of the hundreds of background tests from which Fig. 1 was evolved and the "law" was formulated were

on specimens cast in the identical type of split-pipe molds, resting on machined metal base plates that were current then and, for the most part, still are. The range of Abrams mixtures was presumably and allegedly all-inclusive extending from the stiff to the fluid and from neat cement to lean concretes. The freshly-cast Abrams specimens certainly bled and segregated just as did similar specimens cast a few years later in other laboratories.

Is it not a strange form of rationalization that would "adjust" 1925 results to account for lack of agreement with "uncorrected" evidence secured a few years earlier? While the matter of water separation still introduces uncertainties regarding the net free-water content as the initial set or stiffening started (flowability, sedimentation, or mobility ended), it must be clear that under the conditions of testing that were current, the moisture-correction approach was erroneous and wholly invalid unless it had also been applied to the original Abrams tests.

Still another misleading aspect of the water-separation rationalization was mentioned in the author's closure to the Gonnerman discussion of Reference 17. Let us illustrate by assuming that a given, fairly stiff (low slump), concrete is 50 percent coarse aggregate and 50 percent mortar. If the concrete is wet-screened the mortar still has the same water-cement ratio as did the concrete but it is now relatively fluid having a high slump. The cement factor (and therefore the water factor) is twice as great for the mortar as it was for the concrete. To accomplish a given change in the W/C ratio there would have to be twice the water separation from a 6 x 12-in. cylinder of the mortar as from a 6 x 12-in. cylinder of the concrete. Thus the sizable visible water separation that occurs in the fluid mortar mixture is less potent per unit of water in altering the W/C than is the less noticeable smaller separation for the stiffer concrete mixture.

This and related aspects were considered at length in such exchanges as the previously mentioned Gonnerman discussion and closure of Reference 17 and the Lyse discussion of the Powers paper.[19] To pursue the matter further here would border on mere reiteration. Moreover, it would be equally repetitive to reopen discussion of the McMillan suggested wet-screening technique of Reference 7, mentioned by Dr. Lyse and also on pp. 1306-1307 of the paper. This too was discussed exhaustively.[7] Dr. Lyse's defense of the McMillan suggestion, seems inconsistent with some of the admissions of the opening paragraph of his discussion.

Despite such differences the author is in hearty accord with Dr. Lyse's deserved tribute to the excellence of McMillan's book, *Basic Principles of Concrete Making*,[21] although, as pointed out on pp. 1298-1299 in a footnote and in the text of the paper, there are some queer inexplicables in McMillan's plotted portrayal of the W/C relationship.

In his studies the author has always had in mind the normal inevitable fluctuations inherent in any experimental venture. In his check testing and analyses he has not been looking for quibbles but rather for departures from the "law" that were well in excess of anything amenable to oral or paper rationalization. He believes that he found them, not once but repeatedly. Nor can such spectacular differences as those of References 13 and 14 be ignored or bypassed any more than can the more recent findings of Walker and Bloem.[8,9,10]

Although it seems clear that Dr. Lyse and the author do not yet agree entirely regarding bases for, or the extent of, some of the major limitations of the W/C versus strength generalization, he is grateful for Dr. Lyse's discussion and gratified that there now seems to be agreement on the major premise: *that the water-cement ratio is by no means the sole significant criterion for the strength that can be developed by a concrete mixture.*

In his broadly retrospective contribution Mr. Spindel opens up various aspects for study and review, most of which go somewhat beyond the intended scope of the paper.

As mentioned on p. 1287 and discussed in some detail in Reference 11, the voids-cement ratio of Feret, and of Talbot and Richart are essentially identical with the water-cement ratio of Abrams. All three approaches allocate to the coarse aggregate or to the total aggregate the role of inert filler. Most of what has been said in the paper applies equally to the voids-cement concept. Mr. Spindel should note also the discussion of Mr. Woods and the additional references he has supplied.[34-37]

One point in Mr. Spindel's discussion is believed to need comment. It is only the 2 or 3 percent of incidentally entrapped air that are disregarded in the W/C approach. As pointed out,[11] the free water (capillary water) at the time of stiffening (initial set), constitutes the major portion of the voids (air plus free water) and it is this fact that makes the water-cement ratio and voids-cement ratio virtually the same things, despite differences in the units employed and in the techniques and terminologies of their respective applications.

Some aspects of Messrs. Walker and Bloem's proposed rephrasing of Item 5, p. 1309, have already been considered in connection with Mr. Cook's discussion. Presumably implicit in their proposal but not stated, is the limitation to workable, mixtures, and adequate placement. The author believes that this qualifying stipulation should be included. He also believes that both the shape of the particle and stiffness of the material constitute essential additions to each of the formulations of which they are not already a part.*

*Since the property of the concrete here under consideration is strength (resistance to applied loads at ordinary temperatures) the aggregates are limited to those normally considered suitable for concrete. This accounts for the omission of any specific mention of such other properties as coefficient of thermal expansion, chemical reactivity, and volumetric instability under moisture changes.

Stiffness was sufficiently discussed in the closure to Mr. Cook's discussion. Shape of particle is significant because of its bearing on the orientation of possible planes or surfaces where slippage may occur and on minimum cross sections vulnerable to fracture by bending or otherwise. Shape of particle also influences the wedging and related stress-raiser aspects of the heterogeneity that accompanies the random structure and arrangement that relates in part to shape. These are readily visualized as one mentally compares particles that tend toward the spherical, cubic, triangular, flat, or elongated.

Since the author does not now accept in its entirety, his own or either of the other two formulations, his revised proposal is appended at the end of this closure with the hope that, if not deemed acceptable, it will invite further constructive alteration.

The author strongly suspects that the primary or initial failure of usual concretes is, for normal aggregates, a slippage at the interface between the matrix and the larger particles of the aggregate. Because the specific surface (surface-volume ratio) of the smaller particles exceeds that of the larger particles, the larger particles will always be the more vulnerable ones against such slippage.

The presence of some fractured particles of sound aggregates across a severed section of the concrete generally indicates that the ends of the particle were so well embedded that it was simpler (less work) for the fracture to traverse a minimum section through the particle than it would have been to follow the interface around it. Such manifestations do not indicate, as is so often remarked, "that the paste or matrix was necessarily stronger (or as strong) as the aggregate." Nor does such a fractured fragment of aggregate preclude a liklihood that the initial (triggering) cause of failure was not still a slippage along the interface of that or some other particle. (It is usually difficult to judge from the ashes or the wreckage just where the fire or the collapse originated). The occurrences within a loaded specimen of concrete are something regarding the details and sequences of which we know little.

The author has not yet studied the references supplied by Professor Welch[29-33] supporting the seeming United Kingdom versus United States conflict of evidence on relationship of aggregate-cement ratio to strength. Unquestionably further study and/or testing will resolve the apparent difference but in the meantime the author agrees with Professor Welch that here is one aspect, which for the present, requires a question mark.

The five references constitute desirable added documentation and provide a starting point for the sort of supplementary study that sooner, or later, must be accorded to this aspect. The author is encouraged that the experience of Professor Welch in his drastically different environment, lends support to the primary thesis of the paper.

The author knows Mr. Wheeler as one of the staunch dissenters who has long and consistently questioned the validity of the unqualified W/C pronouncement. The recital of long-time experience adds its quota to the current theme.

Although by the calendar, Mr. Wood's discussion was the first one received, it seems fitting that (by virtue of the alphabet) it should occupy the closing spot. Mr. Wood's discussion, along with the four appended references[34-37] rounds out in a highly desirable manner the paper and its documentation.

While on the subject of references the author wishes to add one more.[37] This timely article (brilliant for its day) falls in the same time bracket as does the work of Abrams and his then assistant and junior coworker Stanton Walker. The article by the late Nathan C. Johnson clearly recognizes and graphically points up the devastating aspects of excess wetness in ways that parallel Abrams notable work and illustrate important aspects of the water gain phenomenon of Reference 15. While Johnson does not actually propose a water-cement or cement-water ratio as a criterion for the control of mixtures, he does so in essence and his dissertation on the seriousness of excess water merits a place among major contributions to better concrete practices.

The synopsis of p. 1287 states: "Finally a modified duly restricted and qualified version of a W/C versus strength relationship is proposed."

Since, in the light of items mentioned in the closures on the Cook and Walker-Bloem discussions, the initial proposal (Item 5, p. 1309), has been altered slightly, the wording for the proposed W/C versus strength pronouncement is now made to read as follows:

> For a given cement and acceptable aggregates, the strength that may be developed by a workable, properly placed mixture of cement, aggregate, and water (under the same mixing, curing, and testing conditions) is influenced by the:
>
> (a) Ratio of cement to mixing water
>
> (b) Ratio of cement to aggregate
>
> (c) Grading, surface texture, shape, strength, and stiffness of aggregate particles
>
> (d) Maximum size of the aggregate

In conclusion the author again expresses appreciation to the ten contemporaries who have pooled with him their observations and experiences in an effort to clarify thinking and improve practices with regard to this: "The most versatile of all construction materials."

REFERENCE

37. Johnson, Nathan C., "Water the Chief Factor in the Making of Good Concrete," *Engineering Record*, V. 74, No. 27, Dec. 10, 1916, pp. 790-793.

Discussion of a paper by Harry H. Edwards:

Precast and Folded Plate Slabs*

By C. J. POSEY†

Introduction of improved methods of construction of reinforced concrete folded plates, such as those described by the author, will surely enhance the competitive position of concrete as a building material. Interest in this novel form is so great that one frequently sees it imitated in other building materials, most often not even arranged to take advantage of the structural principle of the folded plate.

The demand for folded plate designs would seem to exceed the supply of designers willing to venture into V's and W's. The author has helped to remedy this situation by pointing out that folded plate design can be quite simple, and need not follow the complicated methods of analysis advocated by those who emphasize the lateral bending moments at the folds. Experimental evidence points to the fact that these moments can safely be ignored except in a special case which can be avoided. A more critical question is the upper limit of the ratio of width of plate to thickness of plate. The author's test in which an ultimate load of over five times the design load was reached with a width-thickness ratio of 51 to 2½ represents a value that surely required most skillful precasting and prestressing. It would be of interest to have further details of the author's test, particularly the method of loading and of support.

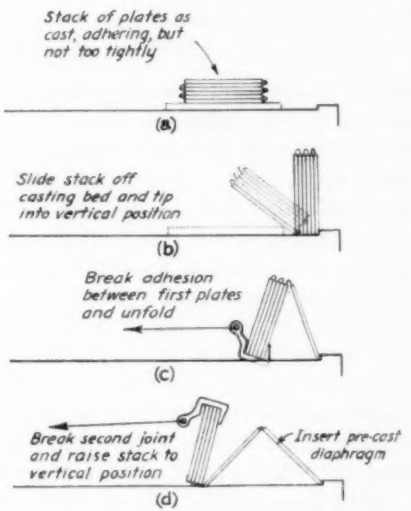
Stack of plates as cast, adhering, but not too tightly
(a)
Slide stack off casting bed and tip into vertical position
(b)
Break adhesion between first plates and unfold
(c)
Break second joint and raise stack to vertical position
Insert pre-cast diaphragm
(d)

Fig. A—Method of casting and unfolding plates

In an attempt to find a method of constructing folded plate roofs without expensive formwork and difficult casting, the writer devised the scheme shown in Fig. A. No heavy hoisting equipment is need-

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1313.

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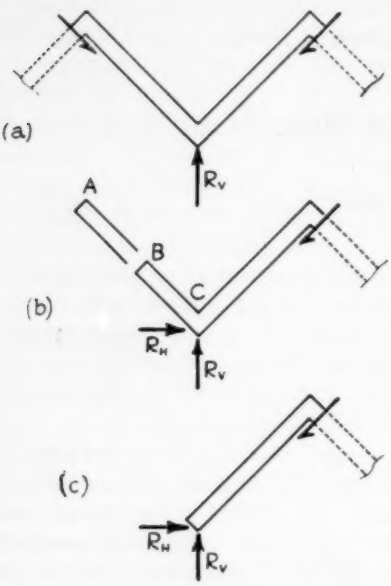

Fig. B—End reactions of folded plate spans (the support must supply an unyielding reaction R_H)

ed. The writer is not familiar enough with pretensioning techniques to know whether prestressing could economically be used, nor the limit of size that could be handled. Students at the University of Iowa built a 10-ft roof by this method, and no difficulties were encountered that would seem to preclude going to considerably greater dimensions.

The writer has observed a mode of failure which, if not adequately provided against, can severely limit the carrying capacity of a folded plate span. Circumstances that permit this type of failure are illustrated in Fig. B. The shears at the top of the interior plates constitute loads on thin inclined beams. The reactions at the ends of these beams are large and must be inclined, since the thinness of the plate precludes any large force in the other direction. Where two interior plates meet, the horizontal components of their reactions constitute action and reaction (in the sense of Newton's third law) and only the combination of the two vertical components needs to be resisted by the supporting wall or beam [see Fig. B(a)]. At the edge trough, however, the inclined end reaction of the edge plate is small compared with that of its neighbor on the inside of the trough, so that a reaction R_H must be supplied by the supporting beam or wall. This is shown in Fig. B(b). The same requirement holds when the edge is finished as shown in Fig. B(c). The support must not only have the necessary strength, but it must be able to supply the required force R_H without appreciable yielding. In the author's Fig. 3 the supporting tie beam does not seem to extend far enough to provide this support. In Fig. 4, the force is not concentrated at the ends, but is spread out along a wall which would undoubtedly be stiff and strong enough.

Returning to Fig. B(b), it is seen that extending the last plate up to A is undesirable, since it results in lateral moments at C that cannot be ignored, and moreover puts an unstiffened thin edge into the compression zone of the folded plate beam. If aesthetic considerations require a turned-up edge, it is better to stop near the neutral axis, as at B in Fig. B(b).

The writer has been unable to verify the formula for I given in Fig. 1. A value of $bdh^2/6$ for the moment of inertia of a single V gives the answer the author obtains in his numerical example. Apparently the first term inside the brackets is intended to evaluate the moment of inertia of the plate about its own axis, but this is a value that could only become worth computing when the plate thickness became so great that the structure could hardly be termed a folded plate.

Discussion of a paper by Jack R. Janney and John F. Wiss:

Load-Deflection and Vibration Characteristics of a Multistory Precast Concrete Building*

By K. W. DAY, YESHAYAHAU ETKIN, and AUTHORS

By K. W. DAY†

During 1953-1954 the writer carried out an investigation into the resonant and composite behavior of the type of floor shown in cross section in Fig. A. The floor was composed of 7 x 6 in. prestressed precast concrete joists, clinker infiller blocks and an in situ screed regarded as nonstructural up to the time of the investigation. The prestress in the joists could be varied and different sizes of infiller block were available so that the floor could economically satisfy a wide range of span-load requirements. The purpose of the investigation was to determine whether the range possible on grounds of static loading needed to be limited on account of vibration characteristics and whether it could be extended by taking into account composite action of the screed and joists. The resonant frequency of unloaded single joists, loaded joists and on three sections of screeded floor on a range of simply supported spans was carried out. The floor sections were then tested to failure under static loading. Good agreement with theory was obtained, the actual resonant frequencies in general being approximately ½ cycle per sec lower than the theoretical values.

The results of the investigation, insofar as they affect the present discussion, were as follows:

1. Oscillations having an amplitude of an inch and more could be built up by one man rythmically swaying on floor sections having a resonant frequency less than 5 to 6 cycles per sec. Accordingly, it was decided that the use of the floor should be limited to applications in which a resonant frequency of not less than 6 cycles per sec would be obtained. As shown in Fig. B this limitation was not severe in the case of the joist and infiller block floor although it would substantially limit the application of a floor composed entirely of side by side joists.

2. In all cases full composite action was obtained at working load between the in situ screed and the precast joists. This was shown by both the resonance behavior and the static deflection. It should be emphasized that this occurred even when the top surface of the joists was a smooth, steel-molded face and no bond provision was made.

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1323.

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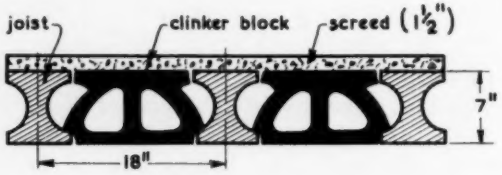
3. Unless the bond is supplemented by shear keys or reinforcement, the composite action obtained under working load does not increase the ultimate strength of the floor. The screed splits off before the ultimate load of the prestressed joists alone is reached.

Comparing these conclusions with those of the authors, it can be seen that although the latter are in no way contradictory, they are open



Details of PIERHEAD PRESTRESSED FLOOR on which tests were made.

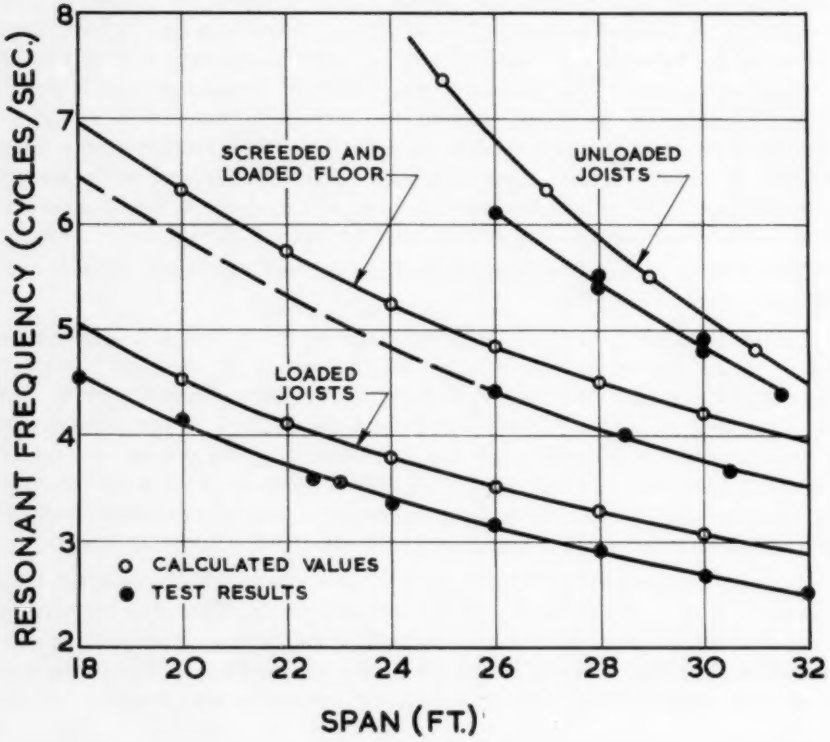


Fig. A—Pierhead prestressed floor details and resonant vibration curves

to quite serious misinterpretation. Thus the first paragraph of the authors' conclusions could (no doubt wrongly) be taken to mean that load deflection and vibration tests at design load are sufficient to establish that full composite action could have been relied on for design purposes. Only tests to ultimate or analysis of horizontal shear stresses at ultimate could do this.

One might also be excused for gaining the impression that since human response to vibration is more sensitive at higher frequencies, a lower natural frequency of a floor slab would be advantageous. Actually, this is a minor factor compared to the susceptibility of the floor slab to the production of vibrations. This is shown by the authors' results, where a floor with a resonant frequency of 20 cycles per sec gave ten times the amplitude of vibration of a floor with 100 cycles per sec resonant frequency and so constituted the worst case. The authors state that "the insignificant vibration caused by walking was sensed by persons standing on the floor whereas the same amount of motion

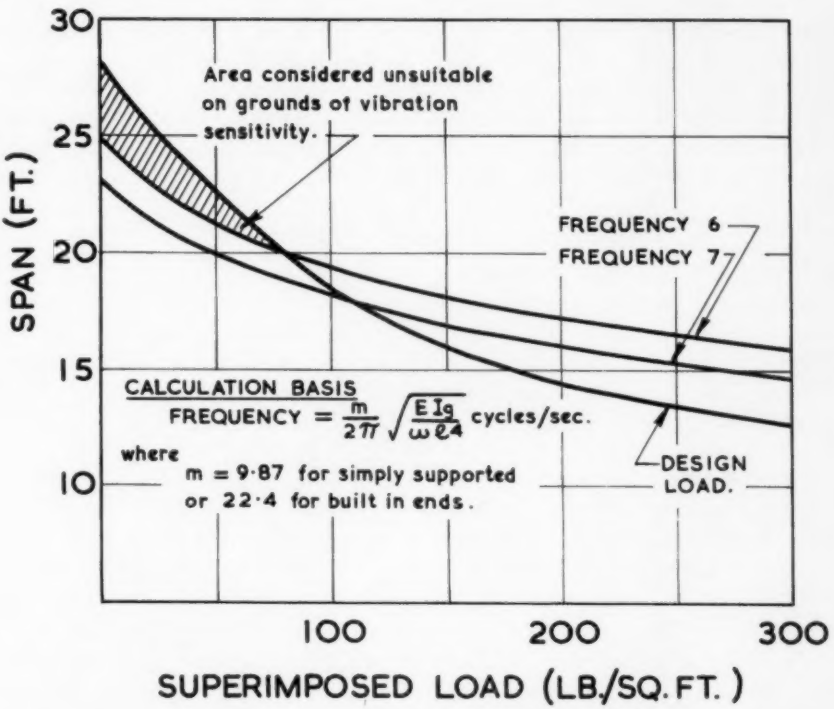


Fig. B—Resonant vibration curves of thin floors (the design load curve shown above is for the pierhead prestressed floor shown in Fig. A; the working moment of resistance of the prestressed joists is 9710 ft-lb)

on a floor of lower natural frequency would not thus be detected." This appears to miss the point of whether or not a substantially higher amplitude of vibration might result from human activity on floors of lower resonant frequency.

The higher modulus of elasticity of high strength concrete is brought in to partly explain the high stiffness of the floor system. This is quite justified and indeed values as high as 7.5×10^6 psi have been obtained by the writer for the instantaneous modulus of high strength concrete. The impression that prestressed precast concrete floors are in general likely, for this reason, to be stiffer than the equivalent in situ reinforced concrete floor, would however surely be incorrect. A reduced depth-span ratio is made possible by high strength prestressed concrete and, if this is utilized, the resulting system would be more flexible than an equivalent reinforced concrete design. Of course in some designs deep ribs will present an acceptable and economical design which will be stiffer than the equivalent plane soffit design.

A possibility which occurs to the writer, but of which he has no information, is that the more rigid and elastic (as opposed to plastic) behavior of high strength concrete may result in less damping through absence of hysteresis. Thus such concrete may present a worse case of vibration sensitivity quite apart from any consideration of frequency.

The writer wishes to thank the Unit Construction Co. Ltd., (England), for permission to publish this information and O. J. Masterman, Technical Director of the company for his guidance during the work.

By YESHAYAHU ETKIN*

As the authors show in their paper, a theoretical, as well as experimental, analysis of structural members subjected to dynamic forces require the evaluation and the most correct knowledge of the flexural rigidity value (EI) of these members. In composite construction we are handicapped in such evaluation because both the I value and the effective E value have to be assessed.

The main factors causing difficulties in assessing them are:

1. Concrete does not obey Hooke's Law. It is always necessary, therefore, to define the range of stress for which a particular modulus of elasticity applies. The modulus is not a constant value, but is larger for low stresses than for high ones. The change in the modulus of elasticity of the particular concrete tested depends on the extent that the stress strain curve departs from the tangent at the zero point.[1,2]

2. When the bending test is performed on structural members which previously had been subjected to either static or dynamic loads, its modulus of elasticity will be higher than that of a beam which was not work hardened by previous loading between the same stress limits.[3]

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The inelastic strains proved to be responsible for the discrepancy between the E_{static} (under static load) and $E_{dynamic}$ (under dynamic load) of concrete. This inelastic strain tends to diminish or disappear under repeated loads.[4]

3. For any particular applied load and stress, there will always be a discrepancy between the E evaluated from a static bending test of the member and the equivalent higher modulus of elasticity which will exist during the vibration unless the recordings of the results of the static test are made instantaneously with application of the load.

These factors, among others, which influence only the computed E value, reduce appreciably the possibilities of getting good correlation between a true calculated and experimentally observed value of the resonant frequency. When the I value also has to be computed, based on the assumed E values and the effective section subjected to the external loads, the analysis is even more complicated.

The writer's experience has shown that even with individual pretensioned, prestressed concrete joists where I was constant, an E derived from the tangent at the zero point to a load deflection curve caused a 10-15 percent lower calculated natural frequency than the observed one.[5]

The joists tested were selected from among typical floor units used mainly for light boarded and screeded floors. They were subjected to periodic external forces under different span and load conditions (no live load, half design load and full design load).

These experiments showed that, for resonance computations of the prestressed concrete joists, usage of the ultrasonic pulse method to evaluate the $E_{dynamic}$, yielded calculated natural frequencies which correspond well with the experimental values. This testing method for evaluating E, by its nature, does not induce any inelastic deformations in the member, and it therefore results in a slightly higher E value than the existing actual $E_{dynamic}$. However, if the equipment for an ultrasonic test is not available, but results of a static bending test can be readily obtained, the writer prefers to use Whittaker's empirical formula[6] to obtain an approximate value of $E_{dynamic}$ for the computation of the theoretical natural frequency:

$$E_{dynamic} = 0.95\,E_{static} + 1.27 \times 10^6 \text{ psi}$$

The writer also tested a slab of two pretensioned prestressed concrete shallow joists (4⅞ x 2½ in.) screeded with 1-in. concrete. In this series of experiments the same basic results as for the individual joists were found. The EI value evaluated from the tangent at the zero point yielded a lower calculated natural frequency than the experimental one. Natural frequency computed by using an effective $E_{dynamic}$ derived from either Whittaker's formula or actual ultrasonic tests corresponded well with the experimental values (−3 to +9 percent).

It will be beneficial to know what the authors assumed for their combined effective E value in both the theoretical computations of the natural frequency and in their experimental analysis with which they derived the high I value mentioned in the paper. It is also of interest to know if their recorded I value in Station 4 was as high as in the other stations. Can the authors substitute in their figures the value of E derived by Whittaker's formula and tell us what is the resultant I value, especially in Stations 1 and 4?

The authors mentioned the reduction in deflections at midspan, and increase of both stiffness and natural frequency of the structural members because the reinforcement over the beam and joint supports served as negative reinforcement in the concrete decking. In the writer's opinion, if it is not purposely and properly designed so, the increase of stiffness of slab due to partial fixity is usually small. In general, the influence of the partial fixity on the natural frequency of a beam when subjected to either a point load or a uniformly distributed load leads to the following:

First, assume we have a fully fixed beam (span $= L$) which is loaded by a point load P at midspan. Then the fixed end moment is

$$M = \frac{PL}{8}$$

But if the beam is only partially fixed at the end

$$M' = k' \frac{PL}{8}$$

Due to the partial fixity the resulting deflection at midspan will be

$$\delta_{\text{simply supported}} - \frac{M' L^2}{8\,EI} = \frac{4\,PL^3}{192\,EI} - k\ \tfrac{3}{4}\ \frac{4\,PL^3}{192\,EI}$$

$$= \frac{4\,PL^3}{192\,EI}\,(1 - 0.75\,k) = \delta_{\text{simply supported}}\ T_1$$

where T_1 is a factor which is a function of the magnitude of fixity k.

Therefore, the natural frequency of a partially fixed beam with a concentrated load at midspan is

$$f = \frac{1}{2\pi}\sqrt{\frac{g}{\delta_{ss}\,T_1}} = \frac{R_1}{2\pi}\sqrt{\frac{g}{\delta_{ss}}}$$

where

$$R_1 = \sqrt{\frac{1}{T_1}} = \sqrt{\frac{1}{1 - 0.75\,k}}$$

By the same method we can find the influence of the partial fixity on the natural frequency of a beam loaded with a uniformly distributed load:

$$f = \frac{R_s}{2\pi}\sqrt{\frac{g}{\delta_{ss}}}$$

where

$$R_s = \sqrt{\frac{1}{T_s}} = \sqrt{\frac{1}{1 - 0.80\,k}}$$

So that if we create unintentionally a small fixity of say 25 percent, the increase in natural frequency will be for either loading condition about 12 percent.

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AUTHOR'S CLOSURE

In reply to Mr. Day's comment concerning the amount of motion (vibration displacement) produced in a floor system by a person walking, it is agreed that a "soft" floor (low natural frequency) will respond with greater motion than a "hard" floor (high natural frequency).

Table 7 of the printed text has the asterisks misplaced. The proper position of the asterisk in the first column (20 cycles per sec) is between the second and third lines (between objectionable and uncomfortable), and in the second column (100 cycles per sec) between the first and second lines (between perceptible and objectionable). As stated in the paragraph immediately before the conclusions, the human evaluation comparison of the two examples indicates that the walking vibration on the upper floors (lower frequency) is more severe than that on the first floor (higher frequency). It must be recognized, however, that since the first floor is a slab on earth fill, it's deflection under walking loads is not directly comparable to a floor system supported on beams and columns.

Although the human evaluation of vibration is a function of frequency, the best information presently available indicates that it varies as the third power in the lower frequency range and as the first power in the higher frequency range. There is also strong evidence to indicate that the human body has a lower tolerance to vibration in the range of 3 to 10 cycles per sec due to the resonant response of individual organs and component systems of the body.

Mr. Day mentions the matter of inherent damping in the vibrating structural system. This factor is certainly worthy of consideration.

Mr. Etkin's comments regarding the elusiveness of determining static and dynamic values for modulus of elasticity are well taken. However, he apparently obtained the impression that efforts were made to establish the modulus of elasticity of the floor system from measurements of frequency and amplitude or to compute natural frequencies from measured or assumed E values. This was not the case.

The authors recognized that an absolutely accurate determination of the elastic properties of the concrete was beyond the range of practicality because the structure was in service at the time of their initial contact with the project, and control specimens were not available. The dynamic characteristics of the floor system were discussed on the basis of measured values of resonant frequency and measured amplitudes, and theoretical predetermined considerations of fixity, modulus of elasticity, and moment of inertia were not necessary for a report of these facts.

For purposes of determining the degree of composite action and discussing extreme fiber stresses and deflections, the moment of inertia was established from measured concrete strains which located the neutral axis of each bending element under consideration. Values of concrete moduli of elasticity were estimated for the same purpose. It should be recognized that the transient nature of the elastic properties of field controlled concrete renders an exact determination of E at each gage location impractical. From a practical engineering standpoint a reasonable estimate of this quantity is sufficient for the stress and deflection comparisons reported. Perhaps its most significant relationship to the discussion on human response to walking vibrations is that as the damping increases, the duration of the transient vibration decreases.

Many static tests to failure have been performed on members similar to those which were the subject of the tests in this report on which composite decks were cast. These tests have shown that the full ultimate flexural capacity is easily obtained without failure between the cast-in-place and precast elements. Furthermore, tests recently performed in the Research and Development Laboratories of the Portland Cement Association have indicated that adequate bond strength throughout all ranges of loading may be achieved without shear keys. It should be pointed out that reinforcement to prevent vertical separation was present in the members under consideration.

Obviously, it was not possible to test the floor in the five-story structure to failure to establish the ultimate load carrying capacity with absolute certainty. The many tests which are mentioned herein with which the authors are familiar led them to feel certain that composite action would prevail through all loading stages.

Discussion of a paper by Walter E. Riley:

Shell Construction – A New Approach*

By MARTIN SCHULZ and AUTHOR

By MARTIN SCHULZ†

The author should be congratulated on a well presented paper. The writer is surprised, however, at the distribution of T_x in Fig. 1. The shell is classified as a "long" shell, $L/b > 1.5$, and for this type the distribution of longitudinal forces is normally close to linear.

Consider first the curve of longitudinal stresses at the intersection of edge beam and shell.[1] In a homogeneous continuum within the elastic law, distortions have to follow a continuous curve. The stresses are related to them only by the modulus of elasticity and by Poisson's ratio. Consequently, they also have to follow a continuous curve.

The total deformation of the cross section of the shell may be considered as composed of two deformations: (1) the deflection of the total cross section along L, considering no deformation in the direction of its transverse axis, which when projected onto a vertical line, is accompanied by a linear distribution of elongation, and because of a constant modulus of elasticity, with a linear distribution of the stresses σ_x; and (2) the additional horizontal and vertical displacements of each point of the cross section, which produce additional linear change. Due to the additional deformation of the cross section, the curvature of the fiber adjacent to the edge member must change continuously in the horizontal projection as well as on the vertical plane, since the shell has to blend into the edge member without a gap and without a turn of the tangents. Therefore, the curve of the stresses of the shell plotted in relation to the height has to blend tangentially into the edge mem-

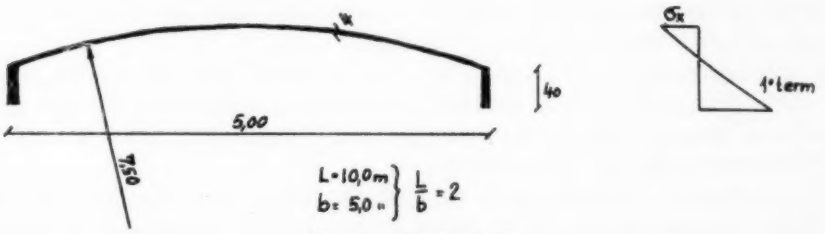

Fig. A—Single shell

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1361.
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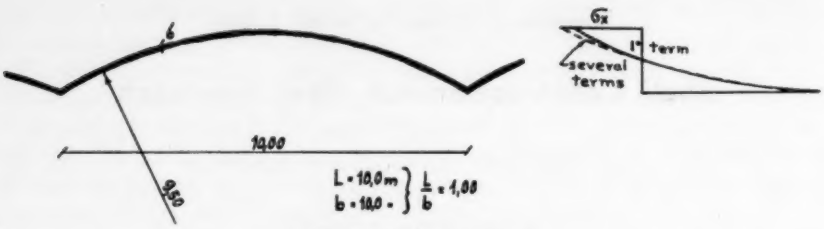

Fig. B—Multiple shell

ber. In other words, there can be no bend. Therefore, the bend is a result of the different mechanical conditions within the edge member and the shell.

Examples of a single barrel with edge members and of a center shell without edge members are shown in Fig. A and B.[1,2] Contrary to Mr. Riley's argument it is obvious from the illustration that at a ratio $L/b = 2.00$, the course of the longitudinal stresses is practically linear if the edge disturbances on both sides are added (see Fig. A). Fig. B illustrates that the deviation from the linear law on an almost short shell is slight and becomes more linear if more members of the series are taken into consideration.

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2. Rüdiger-Urban, *Kreiszylinderschalen*, Leipzig, 1955.

AUTHOR'S CLOSURE

The author wishes to thank Mr. Schulz for his interest in this paper. In reply to his comments it should be noted that for interior shells, of the "long" type the distribution of the longitudinal unit stresses Tx/t is usually close to a straight line. However, for a single bay shell with a relatively shallow edge beam this ordinarily does not hold true due to the horizontal deflection of the shell.

For instance, Fig. C shows a single bay "long" shell with a length to width ratio of 2.0 with longitudinal stresses as calculated by Flügge[3] and also a diagram giving the corresponding longitudinal force T_x. Note the stresses are far from linear.

The author is familiar with the Rüdiger-Urban book[2] and has used its tables several times. As to the example of Fig. A the author agrees that the longitudinal stress distribution is practically a straight line, but this results from the influence of the extremely deep edge beam. For shells of the type described in the paper with shallow edge beams, the

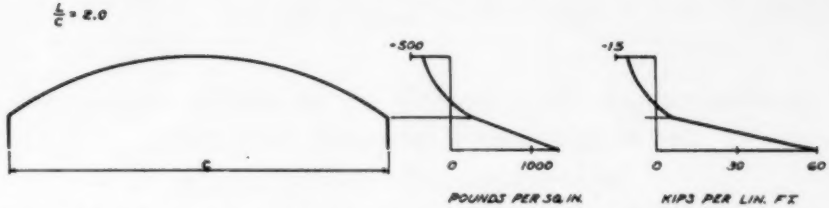

Fig. C—Distribution of longitudinal stresses and longitudinal force in a single bay "long" shell

author finds straight line stress distribution does not theoretically result as stated earlier.

Another example of a single bay shell with relatively shallow edge beams and the resulting curved longitudinal stress distribution is given by Girkmann.[4]

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4. Girkmann, K., *Flachentragwerke*, Springer-Verlag, Vienna, 1956.

Discussion of a paper by Shu-T'ien Li:

Fixed-End Moments in Columns of Asymmetrical Multispan Integral Frames due to Longitudinal Displacements*

By ISMET AKA, TSUNG-LIEN CHOU, ZBIGNIEW M. GLINIECKI, LOUIS T. STACHEL, and AUTHOR

By ISMET AKA†

In the beginning of his article the author states that for symmetrical frames the determination of column fixed-end moments due to longitudinal displacement is relatively simple. In this case if x shows the distances between the axes of the columns and this fixed axis of symmetry, for Column (i), Δ_i will be calculated as $\Delta_i = ctx_i$ and M and H will be calculated from Eq. (1). In this equation x will have a sign and consequently Δ, M, and H will also have signs.

In the asymmetrical case, it will be proved that this fixed point is the centroid of the column rigidities $\alpha = I/h^3$. Therefore, it will be possible to determine this centroid and to calculate M and H as in the symmetrical frames. In this way it will be possible to obtain a simple solution without trying to find a new method as in the author's article.

Now let us prove that this fixed point is the centroid of $\alpha = I/h^3$. If we consider a frame with n columns under the same conditions of the article and use the same notations the horizontal reaction in Column (i) is

$$H_i = 12\,E_c\,\alpha_i\,\Delta_i$$

When I is constant [see Eq. (4)]

$$\alpha_i = I_i/h_i^3$$

The condition for longitudinal equilibrium requires that

$$\Sigma H_i = 0 \qquad \Sigma \alpha_i \Delta_i = 0 \qquad \Sigma \alpha_i x_i = 0$$

If the distances between the axes of the first column and the other columns are shown as x'

$$x_i = x_1 - x_i'$$

Putting this value of x_i in the equation $\Sigma\, \alpha_i x_i = 0$

$$\Sigma\, \alpha_i (x_1 - x_i') = x_1 \Sigma\, \alpha_i - \Sigma\, \alpha_i x_i' = 0$$

*ACI JOURNAL, *Proceedings* V. 57, No. 10, Apr. 1961, p. 1373.
†Assistant Professor, Technical University of Istanbul, Istanbul, Turkey.

and from this equation

$$x_1 = \frac{\Sigma\, \alpha_i\, x_i'}{\Sigma\, \alpha_i}$$

will be obtained

Thus, it is proved that this point is the centroid of the column rigidities shown as α.

When I is variable α will be taken as

$$\alpha_i = \frac{\gamma_i}{12\, E_c}$$

When the frictional resistances at the free-end bearings H_a and H_f are not equal

$$x_1 = \frac{\Sigma\, \alpha_i\, x_i' + \dfrac{H_f - H_a}{12\, E_c\, Ct}}{\Sigma\, \alpha_i} = \frac{\Sigma\, \alpha_i\, x_i' + \beta/ct}{\Sigma\, \alpha_i}$$

This equation corresponds to the first equation of Eq. (7). In fact, if we write $\Delta - \Delta_c = ct\,(l_2 + l_4)$ in Eq. 6, then K_1, K_2, K_3 will become

$$K_1 = [\alpha_3\, l_3 + \alpha_4 (l_2 + l_3 + l_4)]\, ct + \beta$$

$$K_2 = l_3\, ct$$

$$K_3 = \Sigma\, \alpha$$

and $\Delta_1 = x_1\, ct$ will be obtained.

CONCLUSIONS

As explained herein because the problem is reduced to the solution of a first degree equation with a single unknown the result will be obtained in a simpler way and moreover the number of spans will not influence the simplicity of the method.

By TSUNG-LIEN CHOU*

To secure safety, economy and efficiency of a structure, there are four primary functions of design: (1) to study the loading conditions, (2) to find out the nature of the material in the capacity of homogeneity or isotropy, (3) to prescribe and control the process of fabrication and erection, and (4) to investigate the physical theories of stress and strain by laboratory experience and rational analysis. So far, the first three are solved by trial and error, and we can only apply the law of statistics and probability. Function (4) is a key used to check the solutions.

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Consequently, various practical applications are devised with curves and nomograms which simplify nonlinears and other involved expressions. Concrete is cast in a continuous process and metal joints are welded, instead of being riveted or pin-connected. Thus the monolithic and integral frames are more commonly used. Designers have turned from simple to indeterminate structures. The coming of electronic computors will relieve him from the drudgery of slide rules and graphs, but his work is still concerned with deriving the solutions and programing the final computations. Consequently, the theory of elasticity is replaced by the theory of plasticity and linearization by curvilinear relations. Thus today's designer should have a thorough theoretical background and a vivid imagination of what he is doing.

On account of this tendency, the paper is a timely contribution by a prolific author. There are many recommendable points. Instead of using conventional approximations, the author converts his equations into elegant forms, as in Eq. (A) to (D). Nevertheless, the matrix solution is conducive to the electronic computer in which the author is an authority both from a theoretical as well as a practical standpoint.

It may be pointed out that this derivation is based on the geometry of the members and the statics of the structure. Thus, Eq. (A), (B), and (C) represent purely geometrical relations and Eq. (D) expresses a simple statical condition. The development is concise and applicable to the various conditions stated by the author. But in actual construction of heavy structures, columns and girders may have some nonmathematical curves around the joints. Under such conditions, Eq. (12), (14), (15), and (26) can hardly be integrated without resorting to some approximations by arithmetic methods.

The author mentioned the application of the formulas to cover more spans and bays. In principle, this is possible. However, matrix arrays of more than four elements will be complicated, as far as mathematical manipulation is concerned. Also, it is a well-known fact that the joint displacements are cummulatively increasing towards the abutments and the end moments are increasing towards the center of the crossing. In cases of more than five spans, it may prove more economical to break up the continuity by introducing hinges. This is exactly what was done for a seven span bridge frame designed by the writer for the Elahera Irrigation Project of the Ceylon Government.*

As indicated in Fig. A this is a spillway bridge, built across the Heerati Oya and the analysis is further complicated by the multiarch dams on the upstream side to dispose a flood overflow of 5000 cu ft per sec. The arch barrel is circular in form with a uniform rib from

*Chou, Tsung-Lien, "Reconditioning an Irrigation System by Re-assessing the Water Resources in the River Valley and Modifying the Existing Structures with Special Reference to the Elahera Scheme in Ceylon," *Proceedings*, Regional Technical Conference on Water Resources Development in Asia and the Far East Flood Control Series No. 9, United Nations, p. 401.

crown to springing. The central angle is 160 deg, which is the most economical value for a constant angle arch. The arch buttresses are combined with the bridge piers. In the bridge, it was found that by comparing several designs, that two hinges at the central span and free supports provided better economy and easier construction. The rock formation is close to the ground surface, but field tests proved that complete fixity of the footings could only be secured with deep construction. The final design was compromised by taking the bases as semi-fixed. Each frame was composed of three columns and two end spans, corresponding to the author's Case 3.

This practical example may illustrate the deviation of practical construction from theoretical analysis. It also illuminates the importance of engineering theory for guiding practical applications.

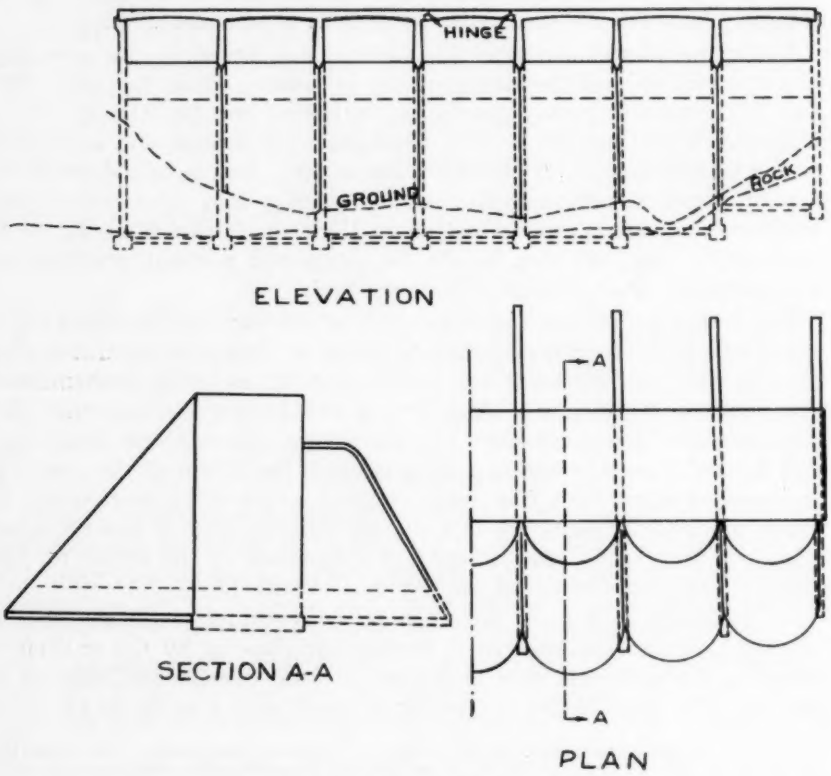

Fig. A—Spillway bridge sections

By ZBIGNIEW M. GLINIECKI*

Dr. Li has presented a lucid and comprehensive paper, particularly valuable to a bridge engineer. The solutions are clearly set out in tabular form for easy application.

In the development for temperature rise it should be noted that deformation due to temperature rise will be resisted by the horizontal reactions of the columns (H_1, H_2, H_3, and H_4), and the frictional resistance at end bearings (H_a and H_f). The resulting horizontal displacement D will be the algebraic sum

$$D = \Delta - \frac{(H_a + H_1) l_2}{A_2 E_c} + \frac{(H_a + H_1 + H_2) l_3}{A_3 E_c} + \frac{(H_f + H_4) l_4}{A_4 E_c} \quad (34)$$

where Δ is an unrestrained displacement.

Similarly

$$D_1 + D_4 = D \quad (35)$$

$$D_2 + D_3 = ct\, l_3 - \frac{(H_a + H_1 + H_2) l_3}{A_3 E_c} = D_a \quad (36)$$

$$\frac{D_1 - D_2}{D_4 - D_3} = \frac{ct\, l_2 - \dfrac{(H_a + H_1) l_2}{A_2 E_c}}{ct\, l_4 - \dfrac{(H_f + H_4) l_4}{A_4 E_c}} \quad (37)$$

and

$$a_1 D_1 + a_2 D_2 - a_3 D_3 - a_4 D_4 = \beta \quad (38)$$

As can be seen the solution is increasingly cumbersome.

The difference in answers using rib shortening is relatively small. In addition it tends to decrease the fixed-end moments in columns. Therefore, the use of this refinement is not necessary in normal engineering design.

One development was omitted from the paper to make it complete, viz., the action of a longitudinal force. But using the pattern developed by the author, a similar array can be set in a tabular form for quick reference.

By LOUIS T. STACHEL†

A general method for determining fixed-end moments in columns of asymmetrical frames due to longitudinal displacements was developed by Dr. Li in a logical mathematical manner and his presentation is well arranged and easy to follow. The writer found no flaws in the definition or description of the method nor were any mathematical discrepancies found in the presentation.

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The method should prove useful to many since it can be used with such versatility for different types of structures. Although other methods can be developed for the same purpose, the method presented will provide the solution(s) easily, especially by programed electronic computation.

AUTHOR'S CLOSURE

The author appreciates the interest shown by the various writers who have contributed to the discussion of this paper and welcomes the opportunity of pondering on the subject further by way of response and expounding.

CENTROID OF COLUMN RIGIDITIES

Professor Aka has given an excellent exposition and conclusion on the centroid of column rigidities as applied to the determination of longitudinal displacements, confirms the author's results, and demonstrates his analytical ingenuity. However, his proof would have appeared much longer if he had started without the author's setting forth of the problem, nomenclature, sketch, and formulation of basic parameters.

A perusal of Professor Aka's round exposition reveals, in particular, a lack of rigorous proof in stating $\Sigma\, \alpha_i\, x_i = 0$ immediately after $\Sigma\, \alpha_i\, \Delta_i = 0$, knowing only that Δ_i are longitudinal displacements at the tops of columns and x_i distances between the axis of columns and the centroid of column rigidities.

It is fittingly convenient to define the quantities, $\alpha_i = I_i / h_i^3$ (or more properly $E\, I_i/h_i^3$, as column rigidities in discussing the present subject. Eq. (7) clearly reveals the centroid of column rigidities without Professor Aka's proof, for by substituting Eq. (6) into Eq. (7) and simplifying the already formulated expressions of Δ's, if $\beta = 0$, we get at once in matrix vectors:

$$\Delta_1 = \frac{ct}{\Sigma\, \alpha_i} [\, \alpha_2 \quad \alpha_3 \quad \alpha_4 \,] \{\, l_2 \quad l_2 + l_3 \quad l_2 + l_3 + l_4 \,\} \qquad (39)$$

$$\Delta_2 = \frac{ct}{\Sigma\, \alpha_i} [\, \alpha_1 \quad \alpha_3 \quad \alpha_4 \,] \{\, -l_2 \quad l_3 \quad l_3 + l_4 \,\} \qquad (40)$$

$$\Delta_3 = \frac{ct}{\Sigma\, \alpha_i} [\, \alpha_1 \quad \alpha_2 \quad \alpha_4 \,] \{\, l_2 + l_3 \quad l_3 \quad -l_4 \,\} \qquad (41)$$

and

$$\Delta_4 = \frac{ct}{\Sigma\, \alpha_i} [\, \alpha_1 \quad \alpha_2 \quad \alpha_3 \,] \{\, l_2 + l_3 + l_4 \quad l_3 + l_4 \quad l_4 \,\} \qquad (42)$$

where each row vector is an array of "effective" column rigidities and each column vector the corresponding array of lever arms in terms of

span lengths. These lever arms are the distances, x_i', between any column whose top displacement is being sought and the other columns.

Thus, it naturally follows from Eq. (9) to (42) that

$$x_i = \frac{\Sigma\, a_i X_i'}{\Sigma\, a_i} \qquad (43)$$

and

$$\Delta_i = ct\, x_i \qquad (44)$$

Clearly, Professor Aka's conclusion is merely a corollary of the author's general solution which embraces the whole exposition of the centroid of column rigidities, and beyond as will be shown in the next section.

SKEW-SYMMETRIC MATRIX FOR CENTROID OF COLUMN RIGIDITIES

By inserting the rigidity of the column of which the top displacement is sought in each row vector, making it common to all, and introducing a corresponding "null" element in each column vector, after fitting up the signs of the elements, we arrive at a "skew-symmetric" matrix for x_i from centroid of column rigidities to column center-lines as:

$$x_i = [\, a_1 \quad a_2 \quad -a_3 \quad -a_4 \,] \begin{bmatrix} 0 & -l_2 & \Sigma_3^3 l & \Sigma_4^4 l \\ l_2 & 0 & l_3 & \Sigma_3^4 l \\ -\Sigma_2^3 l & -l_3 & 0 & -l_4 \\ -\Sigma_2^4 l & -\Sigma_3^4 l & l_4 & 0 \end{bmatrix} \qquad (45)$$

$$i = 1, 2, 3, 4$$

It shows that the mathematical model of skew-symmetric matrix simulates perfectly the physical array of lever arms of the column rigidities of the asymmetrical multispan integral frames under consideration. Mathematics is not merely a tool of scientists and engineers. It reveals the true relationships of things in nature.

The mathematical relations of Eq. (45) is of further suggestive value. By studying the configuration of its elements and vectors, an analyst may extend or reduce the skew-symmetric matrix for more or less spans and hence determine the longitudinal displacements for any composition of integral frames.

REFORMATIVE METHODOLOGY

Professor Chou deserves commendation for the first part of his discussion in advocating urgently needed basic engineering-philosophical awareness. To condense all his timely thoughts, expressed and implied, into the capsule of two words, the author has chosen "reformative methodology."

Present-day magnitudes of engineering works and their complexities demand unprecedented physical recognition, analytical approach, and mathematical formulation, if safety, economy, and aesthetics are to be achieved as an engineer should by virtue of his professional career. This new state of technological challenge is not covered by yesterday's rule of thumb, nor contained in today's handbook, nor adequately met with the trite experience of the so-called practical engineer. Theory and practice go hand in hand. All valid theories are practical. No sound practice is not in conformity with proven theory. To segregate theory and practice is the fallacious weapon of the ignorant.

Arbitrary assumption and unjustified approximation only reveal incompetence. Unless one knows the truer value of a parameter, he will not realize where he stands in his approximations. Take the simple example of π, for instance. Only when one knows its value to eight digits is 3.1415927, he can say its approximate value to seven digits must be 3.141593. Without knowing its approximate value to three significant figures being 3.14, one cannot be sure its approximation to two significant figures is 3.1 plus. Today's complicated problems require knowing bounds of error, necessitating proven approximations. Arbitrary assumption can be disastrous.

Any indeterminate analysis depends on EI, or in a more general way on the stress-strain relation and the moment of inertia. While I's can be brought to within the final degree of desired accuracy by "derivation" or by successive approximation, the role played by the stress-strain relation cannot be more accurate than the accepted experimental results. True as they are, however, rational analysis will guard against the creeping in of further uncertainties and provide the engineer with defendable substantiation for his design.

Not only nonmathematically varying configurations have to be treated by numerical integration, as commented by Professor Chou, but also some of the mathematical curves need the same expediency when their integrals are complicated by the negotiation of another variable function. Regardless of whether integrable or nonintegrable, however, if a computer is used to execute all the calculations, integration is always substituted by special digital techniques with finite ds, however small it may be, and hence integrability makes no difference. In spite of this fact, the best layout engineer invariably foresees analysis by avoiding unnecessary mathematical difficulty and structural ambiguity.

ECONOMICAL COMPOSITION

In the second part of his discussion, Professor Chou has cited the economy and ease of construction in the case of a spillway bridge across the Heerati Oya in Ceylon, by breaking up a seven-span structure into the three-span flanking systems joined by a centerspan freely supported

on hinges. His arrangement fits well in that particular situation especially for the semi-fixed base. The author's five-span composition chosen for the development of his paper is typical in divided expressways with a wide central mall and two end spans to accommodate the overpass earth slopes at abutments. The most economical arrangement in a given situation, if "unrestricted" by a priori requirements, is dictated by the proven criterion that the sum of the cost of superstructure and that of the substructure shall be a minimum. This criterion is generally fulfilled when the cost of superstructure becomes equal to that of the substructure.

FURTHER REFINEMENTS

Mr. Gliniecki's attempt for further refinements is particularly praiseworthy as a rare, fine quality of engineers in contrast with the indifferent attitude held by many a member of our profession. Feeling cumbersome as he is in trying to account for the effect of such secondary deformation as due to temperature rise, his spirit of inquiring into the nature of things is to be encouraged for the advancement of engineering science.

To complement Mr. Gliniecki's constructive comments, the author wishes to pay him a reciprocation by suggesting him a worthwhile extension of the subject, i.e., fixed-end moments in members of asymmetrical multispan integral frames due to vertical displacements.

LONGITUDINAL DISPLACEMENTS

As its title clearly indicates, the author's paper is exclusively devoted to determining fixed-end moments in columns due to longitudinal displacements of asymmetrical multispan integral frames. Such longitudinal displacements are those due to thermal rise, thermal fall, shrinkage, and rib-shortening (compression shortening) — all physical changes or changes due to stress-strain response.

Those due to the action of longitudinal force as Mr. Gliniecki has commented, and, in addition, those due to vertical dead load, live load, impact, and snow load if any, because of asymmetry in geometry and loading, are not within the defined limits of this paper. In fact, all the effects of these longitudinal displacements are automatically taken into account by the established methods of analysis, except moment distribution in which case a supplementary sidesway analysis is required.

POSTLUDE

In perusing through the discussers' pages, the author feels heartened in the unanimous endorsement of the soundness of his method as presented, particularly Mr. Stachel's unreserved, positive sanction. The

author has looked forward to learn better methods from discussers. Professor Aka's touching on the centroid of column rigidities is most fruitful in evoking the author's presentation of the skew-symmetric matrix for lever-arm arrays of column rigidities. Professor Chou's philosophical highlights are practical turning points of the day. Mr. Gliniecki's spirit of refinement is the key of success in confronting new technical challenges. The author extends his deepest appreciation to all of them.

Discussion of a paper by Neville S. Long and Thomas W. Howell:

Concrete for the Mammoth Pool Power Tunnel*

By LEWIS H. TUTHILL, M. R. VINAYAKA, and AUTHORS

By **LEWIS H. TUTHILL**†

We are indebted to the authors for a paper which gives considerable detail on methods followed on a large modern power tunnel built by a private owner. While many of the procedures followed are relatively standard good common practice, viewpoints of the owners concerning uniformity and quality of concrete appear to be somewhat more relaxed than those of some of the governmental agencies concerned with constructing water utilization projects. Perhaps the most conservative of these is the Board of Water Supply of the City of New York, which used 8 sacks per cu yd in lining the Delaware Aqueduct tunnels.‡

Unfortunately much of the data presented included only average values of aggregate gradings and concrete strength. The picture of either performance is incomplete without also maximum and minimum test results and coefficients of variation in strength. For the cement content reported, most of the average strengths in Table 2 seem low, especially with use of a water reducing admixture. Clearly some remarkably low coefficients of variation would have had to be achieved if a commonly acceptable number of tests did not fall below the strengths specified in Table 1.

In view of the authors' comment on such a criterion on p. 1456, coefficients of variation were evidently not low, at least for the invert concrete. If a 3000-psi average was required to prevent more than 10 percent of the tests from falling below 2000 psi, the coefficient of variation was about 26 percent. If so, it is probable that 46 percent of the tests fell below 2000 psi when the average was 2062 and that 28 percent fell below when the average was 2350, with some, as the authors mentioned, below 1500 psi (see Fig. 3§). Even though strength was not considered a prime criterion, presumably as might be determined from a predictable service stress, it is a general measure of quality, durability, resistance to leaching and abrasion, etc. It is therefore not clear how concrete of such low and variable strength could be regarded as satis-

*ACI JOURNAL, *Proceedings* V. 57, No. 11, May 1961, p. 1441.

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‡Clark, Charles M., and Spann, George, "Tunnel Lining Practice on the Delaware Aqueduct," ACI JOURNAL, *Proceedings* V. 37, Feb. 1941, p. 330.

§ACI Committee 214, Recommended Practice for Evaluation of Compression Test Results of Field Concrete," ACI JOURNAL, *Proceedings* V. 54, No. 1, July 1957, pp. 1-19.

factory to produce the long range serviceability which ordinarily would be regarded as of first importance in such a structure, and not worth jeopardizing for relatively minor savings in construction cost.

Could it be that factors causing this variation similarly affected the reinforced lining and structural concrete? If so, it appears that 38 percent of the tests of the lining (average 3260 psi) could have been less than the specified 3000 psi and that 27 percent of tests of the structural concrete could have been less than 3000 psi although it averaged 3580. Portions of this concrete averaging 2810 psi must have had most of its tests less than the 3000 psi specified.

It is not understood what portion of the lining is "backfilled." This is not shown in Fig. 10. Could this be any lining which did not require reinforcement? If so, it is surprising that only 2000-psi concrete would be specified for it, and fortunate that it averaged 2800 psi despite the variation which may have characterized this concrete also.

If indeed variation existed as may be inferred from the author's statement concerning the invert concrete, it might be worthwhile to speculate on the cause. It is suggested that it was in variability of grading, and in variable amounts of fines and undersize in the fine and coarse aggregate as batched. No data was included by which this source of variability can be judged but in view of the way fine aggregate was made, it is not unlikely.

The average grading of the sand does not appear to have been too questionable a factor in view of using air entrainment, and omission of the rod mill cannot be questioned from the data presented. But if the grading varied widely, particularly in amounts passing No. 100 and No. 50 screens, use of a sand sizer to improve uniformity could well have been justified for its contribution toward a more uniform concrete in the tunnel lining.

An arrangement whereby the owner supplies the aggregate is notorious for its inflexibility when it comes to using special procedure to improve uniformity and cleanness when such needs appear, as they usually do, during the concrete operations. Such arrangements have almost invariably proved inimical to effective concrete engineering. Apparently, stockpiling was necessary, in view of the difference in aggregate plant and batch plant production rates. Uniformity of such material from stockpiles would have been greatly improved by finish screening and probably by rinsing on those screens. No mention was made of this provision if it existed.

It should be recognized that there was little the authors could do to correct such questionable arrangements once the job had started.

By M. R. VINAYAKA*

The authors are to be commended for the lucidity with which the paper has been presented, particularly with water control methods and concrete materials adopted in this job. After studying the article in detail, the writer feels that clarification on the following points would help engineers and technologists handling similar jobs to appreciate the article more fully.

1. In the lined section of the tunnel both in the horseshoe and in the reinforced circular concrete sections, it is presumed that concrete was placed only by pumping through an 8-in. line. Was the concrete deposited directly at the arch and allowed to drop down on the sides or was the pipeline required to be shifted from the sides gradually to the center of the arch?

2. How was the segregation prevented if the concrete had to drop along the sides of the tunnel from the center of the arch?

3. What lengths of shuttered linings were taken at a time for concreting? Was the work continuous? If not, how were the day-joints of construction joints handled?

4. Was the pipeline continuously embedded in concrete as recommended in the U. S. Bureau of Reclamation Concrete Manual for placing arch concrete and was there no difficulty experienced in retracting the same as concreting proceeded?

5. How was 100 percent consolidation ensured both at the arch and side wall concretes? What was the spacing of the windows for vibration and how far were they effective in covering the entire concrete placed, particularly in the arch?

6. It is mentioned that form vibrators were used on the steel forms. Keeping in view the thickness of the shutter lining, to what depth can these surface vibrators be effective in consolidating concrete?

7. Was the use of a pneumatic placer considered for shooting the concrete instead of using the concrete pump? Why was the placer not preferred to a pump?

8. Placement in the reinforced circular concrete sections should have been a real tough job. Ill effects due to heavy segregation would have posed some problem. How was the situation faced?

9. Has any grouting been done in the arch of the concrete lined sections? If so, what was the order of intake of cement grout? Does it confirm that placement and consolidation has been reasonably satisfactory?

10. Under "Concrete mix data," the authors state that "However, strength was not the primer criterion in this concrete, and it is believed that the results obtained were satisfactory." Keeping in view,

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the strength is related to durability and impermeability, what else were the prime criteria for this job? Can the authors state the coefficient of variation for the 28-day strengths obtained on the test specimens cast at the batching plant?

These clarifications have been asked for, since information on such local problems are not easily obtained by reference to contemporary literature. By such means as the discussion of such articles it is expected that useful information can be disseminated for the advantage of the profession. For this, the writer is thankful to ACI also for providing a free forum for discussion and to the authors for clarification of such specific points. The writer hopes that the authors will not treat the clarifications asked for as beyond the scope of the article published by them.

AUTHORS' CLOSURE

The authors are deeply gratified at the interest shown in this paper and wish to thank those who have contributed to the discussions.

The discussion by Mr. Vinayaka consists primarily of questions asking for additional information about the methods used in the construction of the project. The following information is presented in an effort to provide answers to Mr. Vinayaka's questions:

1. In the sections where room was available the slick line was moved from side to side until the sides were full and the concrete could be deposited in the arch. The concrete was then allowed to fill the arch adjacent to the line until the end of the line was buried sufficiently to permit the utilization of the maximum available pressure in forcing the arch concrete into the voids and in densifying the concrete.

2. The methods used to reduce segregation were (a) to insure the concrete contained the specified amount of entrained air when placed, (b) to insure that slumps were within the specified range, and (c) to place the concrete so that the slopes of the fresh concrete were not too great, thus inducing the concrete to slough and slide rather than allowing it to roll down the slope in a manner which would induce segregation.

3. The steel lined sections were back filled in one continuous around-the-clock operation without any construction joints. The reinforced and non-reinforced concrete lined sections contained vertical construction joints when the length of the section exceeded the length of the forms available. In these cases a vertical bulkhead was constructed at the end of the forms. Stripping was permitted within 12 hr after placment. The vertical joints were then wire brushed and washed before the form was collapsed and moved ahead to the next section.

Transitions were provided at the end of each lined section. The transitions were rough formed when the concrete was originally placed and were given their final shape by the application of shotcrete.

All operations were on an around-the-clock basis so no problem was presented by placements which took longer than one shift (8 hr) to complete.

4. Yes, with the exception mentioned in Question 1. No difficulties other than those normal to this type of work were experienced.

5. By application of the methods and control referred to in the answer to Questions 1 and 2; by continuous visual inspection of the operations; by sounding (hammering) the arch and by drilling and grouting the arch after placement of the concrete. It is to be expected in this type of work that consolidation would be greater at the bottom and on the side than it would be in the arch. There were no windows in the arch, windows in the side were approximately 6 ft apart and were in two rows approximately 4 ft and 8 ft above the invert. Although space was limited there was generally room for a placing crew to work between the forms and the rock.

6. The primary purposes of the form vibrators were to consolidate the concrete immediately adjacent to the forms and to work out as much of the trapped air as possible thus reducing air bubbles in the finished surface. Internal vibrators were always used in conjunction with form vibrators so that consolidation was never dependent on the form vibrators.

7. This is a question of judgment and while the authors are aware of many projects that have been satisfactorily constructed using "pneumatic placers" the Southern California Edison Company has experienced problems with projects where a "pneumatic placer" was used; hence the specifications required the use of the "concrete pump."

8. In the most heavily reinforced section (see Fig. 10) Mix 1B-3P, Table 3, p. 1456, was used. In this mix the maximum size of the coarse aggregate was reduced to ¾ in. and the cement and air content was increased. This was done to insure a plastic free flowing concrete that would flow readily into all voids. In all other reinforced sections mixes containing 1½-in. aggregate were successfully used.

9. The arches of all circular concrete lined sections were grouted. With the exception of one section where the form bulkhead blew out resulting in a large void, the grout take was minimal indicating that placement and consolidation of the arch concrete had been quite satisfactory.

10. The statement quoted refers only to the invert concrete. It was not meant to imply that strength was of no consequence; however, experience in a large number of tunnels particularly with those belonging to the Southern California Edison Company indicates that strength has little meaning in invert concrete. Because of its protected location, there are no problems of weathering or severe shrinkage. The invert lining is not subject to any major stresses and was not placed for structural reasons. The purpose of the invert concrete was to improve

the flow characteristics in the tunnel and to prevent the transportation of tunnel muck fines to the turbines. The coefficient of variation on the test specimens cast at the batching plant was not developed.

Mr. Tuthill's comments are indicative of his usual thorough analysis of anything dealing with concrete.

It is probably true that private owners are more cost conscious than most governmental agencies, however, an approach which did not recognize total cost throughout the life of a project would not be realistic. It is believed that due consideration was given to total costs in the planning for this project.

The Mammoth Pool Power Tunnel concrete specifications were based on long experience with concrete production in this particular geographic area. Strengths of concrete made with crushed granodorite aggregates from this area are generally notably low. All of the suggestions made by Mr. Tuthill have been tried in the past without significant improvement in concrete strengths, hence the additional cost of refinements beyond those used did not appear to be justified.

The references in the text to backfill concrete should have been expanded to more accurately describe what was meant by this term. The backfill concrete was principally concrete fill around pipe connections to the tunnel (see Fig. 10). The requirements were for 3000-psi concrete in water cut-off sections but only 2000-psi concrete elsewhere where the the steel liner pipe took the full water pressure. Cement content was adjusted to fit the requirements of the particular location.

It is felt that on this subject, where the production of the aggregate was under the control of the personnel who were responsible for the concrete quality, the use of owner supplied aggregate represented the best means of handling the aggregate supply problem. The use of a central aggregate production plant to serve the entire project resulted in significant cost savings. The use of rigid aggregate specifications such as are commonly used by many governmental agencies would have resulted in markedly increased costs without significantly improving the results. Had the total quantity of concrete to be placed been much larger the cost per yard of additional refinements would have been correspondingly less. It should also be noted that the remoteness of the area precluded the use of commercial aggregates for this project as their cost would have been prohibitive.

As stated in the article, a considerable range of variation in compressive strength was expected. The test values for the invert concrete varied between extremes of 2560 and 1230 psi for a cement content of 325 lb per cu yd. The intended average of 2000 psi was obtained. A recent inspection of the tunnel after 2 years of use showed no difficulties with the lining and proved that the judgment in deliberately reducing cement and strength was good.

Discussion of a paper by Charles Wilson:

Prestressed Concrete Beams by Electronic Computer*

By I. R. STUBBS and AUTHOR

By I. R. STUBBS†

Mr. Wilson is to be complimented on his work in programing the IBM 610 computing machine for the design of prestressed concrete beams. There is considerable computational effort required in designing prestressed concrete members so that they behave satisfactorily in practice. Much of this work is repetitious and if the engineer is relieved of this work he has more time to devote his attention to more sophisticated problems.

The writer's company has been using an IBM 1620 machine for some time now to design beams and continuous slabs as well as for building span-load tables for standard sections. The use of a machine makes engineering costs economical.

Most of this company's work has been in the design of buildings and smaller bridges rather than primary highway bridges like the ones Mr. Wilson has been analyzing. The writer's company has normally been concerned with longer spans and lighter loads than Mr. Wilson. This has produced an interesting divergence between the author's approach and the company's. The writer notes that Mr. Wilson devotes considerable computational time to the study of the principal tensile stresses and shears at the ends, quarter points and midspans. This company's program computes the principal tensile stress at the end at the center of gravity of the concrete but the calculation is without a control. To date the writer has not run one solution when the principal tensile stress has been greater than that allowed in the code of practice. The writer would like to ask Mr. Wilson why he computes the principal tensile stresses at locations other than the ends.

Since the beams have been slenderer, this company has always been faced with the problem of creep when the cambers or deflections are excessive. For this reason this company has put controls on the camber and deflection computations. The beam program for example is designed so that the beam is perfectly flat (or has a given camber or deflection) at full dead load at infinite time.

The writer was interested to read that Mr. Wilson takes into account that the modulus of elasticity of the concrete and cambers and deflections change with time. The writer has observed that the camber of a

*ACI JOURNAL, *Proceedings* V. 57, No. 11, May 1961, p. 1459.
†Manager, Computing Center, T. Y. Lin and Associates, Van Nuys, Calif.

beam left in a prestressing plant can gain as much as 200 percent over 2 or 3 months. Too often these effects have to be neglected by the engineer when designing by hand on account of the additional computational effort required.

The writer would be interested to learn how Mr. Wilson applied his live load, i.e., whether it is treated as a moving load or as a series of point loads causing the worst conditions of bending moment in the span. Also the writer would be interested in the outline of his logical flow for this part of the program.

Mr. Wilson is to be congratulated on presenting so difficult a subject so clearly.

AUTHORS' CLOSURE

The author would like to thank Mr. Stubbs for his expressed interest in the paper and for his favorable criticism.

Mr. Stubbs' organization, Hamden Testing Services has acquired an IBM 1620 and plans to apply it to prestressed concrete design. The speed and flexibility of this computer will make possible more extensive and complete studies than were practical with the IBM 610.

In reply to the comments of Mr. Stubbs this discussion will cover investigation of principal stresses and then application of live loads.

As was correctly implied the principal stresses are rarely critical in the design of prestressed concrete. The beams used for highway bridges with spans less than 100 ft have been standardized by AASHO and BPR. For these sections which have properly proportioned dimensions, the problem of excessive diagonal tension stresses has not yet been encountered in the author's office. If, however, non-standard sections are to be investigated, there exists the problem of selecting section dimensions for flange and web. When variations in prestressing arrangements are considered, the effectiveness of each arrangement must be evaluated. The best measure of adequacy of a particular section is a study of the stress conditions in the beam under load. For the web of a beam the most meaningful stresses are principal stresses, in particular principal tensile stress. Therefore, the purpose of the described program's detailed study of principal tensile stress was to obtain a quantitative representation of beam behavior at working loads. To this degree the principal stresses will affect the final design.

Concerning live load, an AASHO truck is applied as a moving load. With shorter spans this is modified for military loading. For the different stress studies this truck load is placed so that it produces the maximum moment or shear at the section under investigation. The program selects the appropriate moment and shear equations by testing the position of the truck relative to the span. Inserting the bridge information (span length, distribution, and impact factors) these equations yield the moments and shears necessary for stress determination.

Discussion of a paper by D. E. Branson and A. M. Ozell:

Camber in Prestressed Concrete Beams*

By HORST BERGER and AUTHORS

By HORST BERGER†

The investigation represents an interesting contribution to the study of plastic deformations of prestressed concrete members and provides a vast amount of highly valuable information. The conclusions drawn by the authors and presented as general recommendations for the designing engineer necessitate a critical discussion of the tests and specific characteristics of the test specimen, since some of the results deviate quite obviously from observations made in other places. The unusually short ultimate creep and shrinkage time (100 to 200 days after loading) and the extremely high prestress losses ($k = 0.30$) call for explanations.

The results of measurements (total strains and camber) on 11 prestressed concrete bridges with spans of 60 to 400 ft and prestressed girders of two buildings over periods up to 7 years as given by Finsterwalder‡ show without exception that approximately one half of the ultimate creep and shrinkage strains occurred during the first year, with each following year showing about one half of the strains of the previous year. Ultimate values of strains and camber were reached after 4 to 5 years. The creep factors were 1.5 for the bridges (mostly near or over rivers) and 2.0 for the buildings. The measurements showed excellent agreement with the camber analyses.

Why is the ultimate camber time in the present investigation less than one fourth of the time as recorded on other structures? The authors explain this deviation from the expected and usual as due to the effects of prestress loss. If we compute the camber producing moments after prestress loss, then we find that these moments are in all but one case still 50 to 70 percent of the initial moments. Only in the case of Composite Beam 2 does the camber producing moment disappear. In all other cases, camber must increase as long as creep is present. The prestress losses do explain the difference in shape between creep curves and camber curves: the camber curves level off faster than the creep curves. They do not explain the short ultimate

*ACI JOURNAL, *Proceedings* V. 57, No. 12, June 1961, p. 1549.

†Member American Concrete Institute, Structural Engineer, Severud-Elstad-Krueger-Associates, New York, N. Y.

‡Finsterwalder, "Ergebnisse von Kriech- und Schwindmessungen an Spannbetonbauwerken," *Beton u. Stahletonbau* (Berlin), 1958, pp. 136-144.

TABLE A—COMPARISON OF FACTORS S_t

Beam	Computed	Test
1,2	0.92	0.93
3,4	1.13	1.18
5,6	1.36	1.39
7	1.28	1.33
9,10	1.26	1.28

camber time. The reason can only be that ultimate creep times were unusually short.

It is this writer's opinion that at least part of the explanation is to be found in the small cross-sectional dimensions of the test specimen, resulting in accelerated creep and shrinkage strains. Late application of loads (after 28 days), water-cement ratio and curing (data not given in the paper), and other factors may have contributed. In the composite beams camber is drastically reduced not only by the reduction of the camber-producing moment, but by the fact that plastic strains cause redistribution of stresses involving partially the moment of inertia of the transformed section. The high prestress losses increase the effect.

To explain the origin of the unusually high prestress losses due to creep and shrinkage, it appears necessary to derive an expression for the loss factor k. The prestress loss k of a stressed steel tendon is the ratio between concrete strains due to creep and shrinkage adjacent to the tendon and the initial steel strain

$$k = \frac{\varepsilon_{c,pl}}{\varepsilon_{s,i}}$$

Using the summation coefficient S_t as defined in the paper, the expression for bonded prestressed concrete is therefore

$$k = \frac{n}{\sigma_{s,i}} (\varepsilon_{sh} E_c + \sigma_L C_t + \sigma_p S_t) \quad \ldots\ldots (21)$$

where

ε_{sh} = shrinkage strain
σ_t = concrete stress due to loads
σ_p = concrete stress due to prestress

Since S_t varies little with prestress loss, $1/k$ is almost proportional to the initial steel stress.

Now it is easy to explain why the beams with the lowest prestress level unexpectedly show the highest losses. The steel stresses used in Beams 1 and 2 (about 44 kips per sq in.) are only 40 percent of the allowable and usual stresses applied for Beams 5 and 6. For economic reasons, such low steel stresses are never used in the design of prestressed concrete members. Freyssinet in his patent in 1936 gave the minimum stress to be used as 57 kips per sq in. The usual stresses today are twice this amount and more. If we apply the stresses as used in Beams 5 and 6 to Beams 1 and 2 (with reduced steel area), the loss will be reduced from 30 to 14 percent. Beams 5 and 6 ($k_u \sim 0.22$) on

the other hand, represent extremes insofar as an almost permanent stress level of $0.64f_c'$ is quite unusual. For practical design purposes, prestress loss can therefore be expected to vary between 10 and 20 percent.

The influence of the prestress loss on camber is given by Eq. (5) and (6). Assuming that the change in E can be neglected, Eq. (6) can be written

$$S_t = -k_t + (1 - a\,k_t)C_t$$

where a is an integration factor.

It has been shown that k_t varies almost linearly with C_t so that it is sufficient to assume $a = 0.5$.
Therefore

$$S_t = -k_t + (1 - 0.5\,k_t)C_t \quad \text{.....(22)}$$

Table A shows factors, S_t, as computed with Eq. (22) in comparison with the actual factors, S_t, as given by the authors. The maximum error is 3 percent. S_t/C_t varies usually from 0.75 to 0.90. Assuming as first approximation $S_t = 0.85\ C_t$, the k factors for the tendons of the beam can be determined with the help of Eq. (21). For unbonded members mean stresses have to be used. The result is used to compute S_t according to Eq. (22) where k_t is the loss in moment due to prestress. If S_t deviates much from the originally assumed value, the computation should be repeated with the new value of S_t.

CONCLUSION

The short ultimate camber time and the high prestress losses as found in the investigation are due to specific characteristics of the test specimens. Prestress losses will usually be much smaller and range from 10 to 20 percent. Their magnitude can be computed with Eq. (21). The summation factor can also be computed with Eq. (22).

The tests have in general confirmed earlier observations and theories. They show, however, that more has to be learned about the influence of size of members and time of load application on the magnitude and the development with time of plastic concrete strains. The authors should be complimented on the precision of their test procedures and the thoroughness with which the results were presented.

AUTHORS' CLOSURE

The observations and discussion of Mr. Berger are gratifying to the authors. The approximation given by Eq. (22) is useful in evaluating the summation coefficient S_t. However, this equation could be modi-

fied slightly to include a closer approximation of the first term in Eq. (6), as

$$S_t = -0.9\,k_t + (1 - 0.5\,k_t)\,C_t \quad \text{.....(23)}$$

This approximation for S_t yields results within 2 percent of the experimental data and provides a convenient one-step procedure, along with Eq. (24), for predicting the total (initial-plus-time-dependent) camber or deflection, exclusive of live-load deflection, for noncomposite prestressed concrete beams.

$$\frac{d^2y}{dx^2} = \frac{1}{E_p I}\left[(1 + C_t)(M_s) - (1 + S_t)(P_t e_s)\right] \quad \text{.....(24)}$$

Eq. (23) differs only slightly from Eq. (22), suggested by Mr. Berger, but is included to account for the effect of increase in concrete stiffness with time in this expression.

Mr. Berger is concerned with the ultimate creep, shrinkage and camber data and the high prestress loss reported (maximum 30 percent). The test period was 100 to 200 days, and the ultimate values were stated in the paper as being estimated from the experimental data. High shrinkage in the laboratory plus the low initial prestress level (25 percent f_{ci}' for the beams with the highest loss percentage) were given in the paper as explanations for the high prestress loss.

With regard to the item of ultimate camber being reached relatively early compared to shrinkage and creep, Mr. Berger states that in noncomposite prestressed concrete beams camber will continue to increase as long as creep continues (which would be indefinitely). The authors submit that this would not be true when and if the incremental prestress loss deflection resulting from both shrinkage and creep became equal to the incremental creep camber.

Discussion of a paper by Inge Lyse:

Durability of Concrete in Sea Water*

By HOMER M. HADLEY, PAUL KLIEGER, BRYANT MATHER, and I. L. TYLER

By HOMER M. HADLEY†

This short paper is admirable. It presents the problem of the deterioration of concrete subjected to freezing and thawing in sea water, submits the experimental results obtained from laboratory tests covering all important variables, and finally lists the conclusions that are to be drawn from the evidence. The opinion of not one prodigious and overwhelming authority is cited.

The writer likes this kind of paper and accepts its findings unhesitatingly.

By PAUL KLIEGER‡

The results of Professor Lyse's freezing and thawing tests of concrete in sea water resemble closely those found in the Portland Cement Association's laboratories with respect to the influence of various chemical de-icers on the ability of concrete to resist scaling. These published data[6] indicated that the most severe scaling took place at solution concentrations of 2 to 4 percent by weight, and that the deterioration was more rapid than that occurring in plain water, thus corroborating Professor Lyse's first conclusion. These tests and field performance data have indicated that for concretes exposed to these solutions, one class of which were various concentrations of NaCl in water, the air content of the concrete should preferably be in the high part of the range normally specified for the particular concrete, and the cement content should be above some minimum specified value to produce a reasonably low water-cement ratio impermeable paste.

With respect to Conclusion 3, the writer would agree that the ". . . length of time of moist curing before exposure to freezing and thawing has only a slight effect on the resistance . . . ," only however if these times exceed the minimum amount of moist curing necessary to attain an adequate level of strength to resist the effects of the freezing and thawing.[3]

*ACI JOURNAL, *Proceedings* V. 57, No. 12, June 1961, p. 1575.

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Conclusion 4 dealing with the beneficial effect of drying is entirely in accord with PCA's experience with respect to freezing and thawing in plain water, and more particularly so with respect to the resistance to the effect of chemical de-icers.

The apparent need for an air content of 10 to 20 percent results from a consideration of the entire set of test data reported by Professor Lyse. However, if we follow the recommendations in ACI Committee 613* with respect to the maximum allowable water-cement ratios for sea water exposure in a severe climate, we should eliminate consideration of at least the mixes having a cement-water ratio of 1.50 ($W/C = 0.67$, approximately 7½ gal per sack). This type of concrete would not be recommended for this exposure. Further, if we take advantage of the fact that precast concrete elements, which are generally the type used for marine works, almost always undergo drying during yard storage prior to use, the necessity for an excessive amount of entrained air further decreases, thus avoiding serious reductions in strength, a not unimportant property of structural concrete. If we consider, therefore, the concretes having W/C ratios of 0.50 or less and whose curing prior to exposure included a period of air drying, those concretes having theoretical air contents of 5 percent or more showed excellent resistance. This is more nearly in accord with past experience in this country.

The freezing and thawing data shown are for concretes described in the note on p. 1579. The maximum size of aggregate used in the concretes is not shown. However, the proportions indicate a fine aggregate content of 50 percent by weight of total aggregate. The maximum size of aggregate used was probably of the order of ¾ in. If this was indeed so, the air content normally required for durability would be of the order of 5 to 8 percent and would so be specified, rather than 3 to 6 percent, particularly for sea exposure. It is of interest to note also that the air content suggested by Professor Lyse was based on the theoretical air contents rather than the actual air contents of the concrete, which, as Professor Lyse stated, were always considerably lower than the theoretical.

REFERENCE

3. Klieger, Paul, "Curing Requirements for Scale Resistance of Concrete," *Bulletin* No. 150, Highway Research Board, Jan. 1956.

*ACI Committee 613, "Recommended Practice for Selecting Proportions for Concrete (ACI 613-54)," ACI JOURNAL, *Proceedings* V. 51, No. 1, Sept. 1954, pp. 49-64.

By **BRYANT MATHER***

Professor Lyse drew six conclusions from the work reported in his paper. This discussion concerns only two of these:

1. The effect of freezing and thawing of concrete in sea water is much more severe than in fresh water.

6. While for resistance to frost action in fresh water the percentages of entrained air for best results ordinarily range from 3 to 6 percent, the percentages giving best durability results for concrete in sea water ranged from 10 to 12 percent. . . .

The most extensive study of laboratory procedures for freezing and thawing of concrete[4] was concerned with the four procedures described by the American Society for Testing Materials,[5] one of which, ASTM C 292, provides for "freezing and thawing in water or brine." The relative effects of brine as compared with water were not studied,[4] since all laboratories using this method used water. ASTM C 292 provides that the specimens may be tested "immersed either in fresh water, sea water, or salt solution" and provides that "when a prepared salt solution is used, it shall contain 10 percent by weight of anhydrous sodium chloride, calcium chloride, or other salt;" but is silent regarding any differences in effects to be expected.

Verbeck and Klieger[6] reviewed and confirmed the work of Arnfelt[7] that concretes frozen and thawed in salt solutions showed maximum deterioration at relatively low solution concentrations. They concluded that the mechanism by which materials such as calcium chloride, sodium chloride, urea, and ethyl alcohol in solution affected resistance to freezing and thawing of concrete was primarily physical not chemical. They noted that, for 1 in. aggregate concrete, previous work had indicated a desirable air content of 4 to 7 percent. Their tests indicated that concrete with 5 percent air performed as well as concrete with 6, 7, or 10 percent air when frozen in calcium chloride solutions of the concentration for maximum severity.

Kennedy and Mather[8] reported on the relative severity of laboratory freezing and thawing tests in fresh water and natural freezing and thawing in sea water at Treat Island, Me., and found that a comparable number of degree-cycles laboratory exposure was more severe than field exposure in 4 of 14 comparisons made but that when the comparison was made on a cycle-for-cycle basis the laboratory exposure was more severe in 13 of 14 comparisons made. Powers,[9] in commenting on these findings, stated:

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Had the laboratory freezing rate not been so high, the Treat Island experiments might have shown whether or not a laboratory test can predict field behavior in the same category. With the laboratory test actually used, it is probable that the paste in some specimens was not fully protected against the effect of freezing at 36 F per hr and that degrees of protection were not equal among concretes made with different aggregates. On the other hand, it is possible that pastes were fully protected against freezing rates occurring at Treat Island. If these suppositions are true, frost action in the laboratory was a more complex process than it was on Treat Island because it involved not only effects of freezing in aggregate particles, but also effects of paste dilation, whereas on Treat Island paste dilation may not have been involved.

Neither Kennedy and Mather nor Powers suggested that the exposure at Treat Island was "much more severe" because the water there was sea water.

The intent of the foregoing comments is to indicate that previous work, as it is known to the writer, suggests that:

1. Freezing and thawing of concrete in solutions containing small amounts of dissolved material of any of several kinds is more severe than freezing and thawing of concrete in solutions of lower or higher concentrations.

2. Freezing and thawing of concrete under natural marine conditions may be less severe than laboratory freezing and thawing in fresh water due to a variety of factors, including freezing rate.

3. It does not seem to have been suggested previously that the parameters of the air-void system of frost-resistant concrete need to be different depending on whether or not the concrete is frozen and thawed in fresh or other than fresh water.

It is hoped that the author will be in a position to suggest an explanation of the mechanism by which a different set of air-void characteristics appear to be necessary for concrete to resist frost action in sea water than are required to impart frost resistance in fresh water. It is also hoped that he may be in a position to confirm or refute the hypothesis that the increased resistance of concrete with 10 to 12 percent air used in his tests to freezing and thawing in sea water, as compared to that of concrete with 3 to 6 percent air, indicates that the concrete of lower air content contained an inadequate air-void system, the inadequacy of which was revealed more strikingly and after fewer cycles of freezing and thawing in sea water. This hypothesis suggests that an abnormally large proportion of the total air content in these concretes must have been in the form of ineffectively large voids. This point could be resolved by bubble-spacing factor data of the sort obtainable by the use of ASTM C 457-60T.

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9. Powers, T. C., "Basic Considerations Pertaining to Freezing and Thawing Tests," *Proceedings*, ASTM, V. 55, 1955, pp. 1132-1155.

By I. L. TYLER*

Professor Lyse's six items in the conclusions to his paper should be of great interest to all those concerned with marine construction in severe exposures of freezing and thawing. Items 1 to 4 inclusive and the first part of 5 would probably cause little contention in the United States except possibly for Item 3 in which the beneficial effects usually attributed to adequate curing before exposure appear to be given less consideration than is our custom. Certainly the first part of Item 5 cannot be debated.

It is in the last part of Item 5 and in Item 6 that Professor Lyse has introduced a heretofore unconsidered phase of the sea water exposure problem. PCA's experience has been confined to entrainment of only the common 3 to 6 percent of air in full scale structures and results have been about in accord with Professor Lyse's tests. At 20 years' exposure the high cement-water ratio non-air-entrained concrete mixes in the severe sea water exposure of the long-time study of cement performance in concrete have been damaged appreciably, with the air-entrained mixes faring somewhat better but with nowhere near the improvement found in other types of exposure. This may be in corroboration of Professor Lyse's requirement of high air content.

Professor Lyse suggests that the cement-water ratio effect is small with non-air-entrained concrete. PCA's experiences do not bear this out in the full scale exposures.

The author has contributed a new view on the problem of sea water exposure. It deserves consideration.

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